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THE SPECTRUM OF HYDROGEN GIVEN BY THE METALLIC ARC OF TIN, COPPER, SILVER, ETC.

By O. H. Basquin.

The Problem.—The arc spectra of those elements which are gases at ordinary temperatures and pressures have not been extensively studied. Their spark spectra, however, are easily obtained and were among the first to be investigated. The general impression prevails therefore that these elements do not possess arc spectra. On the other hand practically all the so-called "hot stars" and all the "new stars" possess the more important lines of the hydrogen spectrum. Although our knowledge of what is going on in the arc and in the spark is very crude and unsatisfactory, yet it is, to the average mind, much easier to imagine a star as being in a condition similar to that of the arc, rather than in one similar to that of the electric spark. It has seemed worth while therefore to search for the more important lines of hydrogen in the arc spectrum. This is the problem of the following investigation.

Historical.—Liveing and Dewar[1] examined the carbon arc in an atmosphere of hydrogen and saw "the fairly bright" *a* line

[1] *Proc. Roy. Soc.*, **30**, 156, 1880.

of hydrogen, also "a faint diffuse band" at the position of the β line of hydrogen. They obtained these two lines also by allowing small drops of water to fall into the arc in air.[1] They found the β line usually obscured by continuous spectrum, becoming visible at intervals only, when, from some variation in the working of the arc, the continuous spectrum was less brilliant. Crew and Basquin[2] incidentally noticed these two lines of hydrogen while working with the rotating metallic arc in an atmosphere of this gas.

Apparatus.[3]—In searching for this line I have employed the rotating metallic arc[4] which enables one to use chemically pure electrodes having little or no chemical reaction with the gas employed. In this arc then one may expect the gas to give off its characteristic radiations with greater intensity than in one where the gas may enter into chemical compounds before a temperature is reached at which it becomes luminous. This arc enables one also to select such metals as do not have strong lines in the neighborhood of the lines sought for, while in the spectrum of the carbon arc there are few spaces not already occupied by lines of carbon or of an impurity.

In the rotating arc, one electrode, either a disk or a rod of metal, rotates upon an axis making about 700 revolutions per minute, while the other electrode has a slow movement of translation toward the axis of rotation. The rotation not only prevents the excessive heating and welding together of the electrodes, but it throws the hot gases to one side, so that the arc has the appearance of a small fan. The part of the flame, thus separated from the poles, is very free from continuous spectrum.

In the apparatus used in these experiments the arc is enclosed in a brass box or "hood" having a volume of about 1½ liters

[1] *Proc. Roy. Soc.*, **35**, 75, 1883.

[2] *Proc. Amer. Acad.*, **33**, 18, 1898.

[3] The construction of the apparatus described below was made possible through the generous consideration of the committee of the American Academy of Arts and Sciences in charge of the Rumford Fund.

[4] CREW and TATNALL, *Phil. Mag.*, **38**, 379, 1894.

and being comparatively gas-tight. The light from the arc issues through a long brass tube closed with a lens at the outer end ; the lens thus forms part of the wall of the hood but is so far removed from the arc that it receives comparatively little of the deposit sometimes formed inside the hood, and hence remains clean.

A stream of gas enters the hood at one stopcock and leaves it at another; a third cock is provided for attachment to a manometer. Although the hood is not absolutely gas-tight, the purity of the gas inside was preserved, in these experiments, partly by the small excess of pressure inside the hood above that outside, and partly by the fresh supply of pure gas constantly running through the hood. The hydrogen used was generated electrolytically and varied in quantity from 10 to 15 liters per hour.

The spectra have been examined both visually and photographically by means of a small plane grating spectroscope and by means of a large concave grating spectroscope.

Observations of hydrogen lines.—The arc spectra of the following metals in hydrogen have been examined:

Aluminium, copper, magnesium, coin-silver, sodium, tin and zinc. With the exception of sodium the arc of each metal shows to the eye very clearly the $H\alpha$ and $H\beta$ lines of hydrogen, and in most of them the $H\gamma$ line comes out with the small instrument very clearly, and indistinctly with the large one. The $H\delta$ line shows only rarely and then to the eye rather indistinctly. The $H\alpha$ line is quite sharp and well defined unless the electric current through the arc is unusually great; it has much the same appearance as the zinc line at λ 6363. The other three lines are always broad, hazy, and ill-defined.

On the photographs taken with the large spectroscope $H\beta$ and $H\gamma$ usually show very plainly, always excepting the spectrum of metallic sodium, while $H\delta$ shows in spectra of tin, silver and copper (Plate I, Fig. 2). On photographs taken with the small spectroscope these lines show more sharply on account of the very much smaller dispersion and the photographs of tin show

the next hydrogen line, $H\epsilon$, quite clearly. Not having found the hydrogen lines in the metallic sodium arc (using copper as stationary electrode), I tried this metal in dry hydrogen also, thinking that in some way the water vapor might have affected the appearance of the hydrogen lines, but I have been unable to detect any of the hydrogen lines in this arc in any way.

None of these hydrogen lines excepting $H\alpha$ is sharply defined. A fairly wide space in the middle of the line has fairly uniform intensity, shading off very gradually and uniformly to each side. The following table gives a rough estimate of the widths, in Ångström units, of these lines as they appear on the photographic plates; the middle of the shading being taken as the edge of the line.

Line	Maximum width	Minimum width	Usual width
$H\alpha$	6	4	5
$H\beta$	65	13	31
$H\gamma$	44	14	26
$H\delta$	32	12	20
$H\epsilon$	faint, same general width.		

It will be noticed that these lines, with the exception of $H\alpha$, are excessively wide, and I think it is for this reason alone that I have been unable to photograph the still weaker hydrogen lines of Balmer's series. They may appear upon the plates but are so wide and so faint that they cannot be detected upon the general shading of the plates.

That these lines are not merely spark lines introduced into these arc spectra by the supposed spark at the breaking of the current through the rotating arc is shown, first, by the fact that they were first observed in the carbon arc; and, second, by the fact that I have seen $H\alpha$ and $H\beta$ quite clearly in the magnesium metallic arc, when the poles were not rotating. The lines produced in the stationary arc have much the same character as in the rotating arc, but there is a large amount of continuous spectrum, appearing as a background, in the case of the stationary arc, so that it would be difficult to photograph the hydrogen lines in this way.

These lines in the arc seem to be due to hydrogen and not

to water vapor coming from the hydrogen generators.[1] This is shown by the following two experiments: (1) I passed the stream of hydrogen through concentrated sulphuric acid and phosphorus pentoxide; and even after the stream of dry gas had been running through the hood for three hours I found the *Ha* line as bright as it was in the damp hydrogen coming directly from the generators. (2) In place of the current of dry hydrogen, I passed through the hood a stream of air bubbling through warm water, so that this air was charged with moisture to about the same degree as the moist hydrogen coming directly from the generators. In this case I was not able to detect the faintest trace of the *Ha* line. Magnesium poles were used in both of the above experiments.

Other methods.—I have examined some of these metals in commercial ammonia gas, such as is used in refrigeration. In this gas the hydrogen lines come out with nearly the same intensity as in hydrogen, when copper or aluminium electrodes are used; no hydrogen lines are seen in the sodium arc in ammonia, although the arc works well, and when tin electrodes are used in ammonia a black dust collects in the atmosphere about the arc to such an extent as to shut off practically all the light within thirty seconds after starting the arc. From the standpoint of convenience and safety, the ammonia gas is much to be preferred to hydrogen.

The copper arc in coal gas shows the *Ha* line very clearly, but the other hydrogen lines are not distinguishable on account of the multitude of comparatively strong carbon lines which the coal gas furnishes in this part of the spectrum.

Following the suggestion of Liveing and Dewar, above referred to, I have tried the rotating metallic arc in air, playing a very small jet of water upon the rotating electrode. In this manner the silver arc works rather more poorly than usual, and resembles a rapid series of small explosions. The hydrogen lines come out clearly, but are rather weaker and more diffuse than in the hydrogen atmosphere.

[1] TROWBRIDGE, *Phil. Mag.*, **50**, 338, 1900.

The copper arc works well in an atmosphere of steam, much better than in hydrogen. The hydrogen lines are nearly, if not quite, as strong in steam as in hydrogen. The electrodes of the arc are slightly oxidized, and have very beautiful colors. In making this experiment a slight alteration was necessary in the hood of the arc. The window through which the light issues is usually as far away from the arc as possible, but it was moved for this experiment so as to be as close to the arc as possible. It was placed at the inner end of a brass tube projecting into the hood, in order that the heat of the surrounding steam and hot air, as well as that of the arc itself, might prevent condensation of steam upon the surface of the window.

CHEMICAL ACTION IN THE ARC IN HYDROGEN.

Historical.—Crew and Basquin[1] have sought to eliminate the radiations due to chemical causes in the electric arc by using chemically pure metallic electrodes and enclosing the arc in an atmosphere of hydrogen or nitrogen. They interrupted the current through the arc about 110 times per second and examined the light of the arc while the current was null. They found in the rotating metallic arc *in air* "a luminous cloud" persisting for several thousandths of a second after the current through the arc had ceased, but they found no such luminous effect in an atmosphere of hydrogen or nitrogen. This seems to show that the cloud is due to chemical action going on in the gases after the electric current has stopped, and that in hydrogen the chemical action is too feeble to be noticed in this way.

Liveing and Dewar[2] found a magnesium "line" at λ 5210 making its appearance in the arc spectrum only upon the introduction of hydrogen or coal gas into the arc. Professor Crew[3] gives a number of lines appearing in the iron arc in hydrogen and not appearing in the arc in air.

Hydrogen-metal Flutings.—With the exception of tin, every metal thus far examined in the rotating metallic arc in hydrogen

[1] *Proc. Amer. Acad.*, 33, 18, 1898.

[2] *Proc. Roy. Soc.*, 30, 96, 1880.

[3] *Phil. Mag.*, 50, 497, 1900.

gives a characteristic set of spectrum lines which are not found in the arc in air. Inasmuch as compounds of hydrogen with some metals are known, I have, in lieu of a better hypothesis, supposed that these lines are due to such compounds formed in the arc. No new isolated lines, surely due to hydrogen, have been found. The following description takes up the metals in the order of the relative intensities of these flutings:

Tin.—No fluting has been discovered due to a combination of tin and hydrogen. There are four lines of intensity ½ on Rowland's scale at approximately λ 3715, 3841, 4245, and 4386, which have not yet been identified. These may be weak tin lines not listed, or weak impurity lines. The deposit which is formed in the hood enclosing the arc is very small in amount and of a greenish color, and consists of very small globules of metal. If this deposit is heated upon platinum-foil in a Bunsen flame it quickly glows and thereafter has a slate color, and if this powder is placed in hydrochloric acid it dissolves when heat is applied and gives off bubbles of gas. If the dark powder, after the first heating, is reheated on foil in the flame, it glows again apparently at a higher temperature than before, and then becomes a very white powder, both of which experiments go to show that the original powder is not metallic tin, but is possibly some combination of tin and hydrogen.

Coin silver.—This metal gives a delicate fluting with first head at λ 3333.86 and running toward longer wave-lengths. There are only about fifty lines in this fluting and they have an average intensity rather less than ½ on Rowland's scale.

Copper.—This metal gives a rather open fluting having the head at λ 4279.77 and running toward the longer wave-lengths. The number of lines in this fluting is about sixty and they are individually stronger than those of the coin silver fluting. This fluting makes its appearance also when an atmosphere of ammonia or of steam is used. The deposit formed inside the hood is rather small in amount and of a brown color. The following table gives the wave-lengths of the hydrogen-copper fluting:

Wave-length	Intensity	Remarks	Wave-length	Intensity	Remarks
4279.77	2	Head	4332.98	1−	
4280.72	1		4335.20	1+	
4281.25	1+	Ghost of 4275 ?	4339.80	1−	
4281.85	1+		4341.98	1+	
4282.48	½		4347.06	1−	
4283.38	1+		4349.13	1+	
4284.15	½		4354.59	1−	
4285.26	1+		4356.73	1+	
4287.58	1+		4364.68	1+	
4290.25	1+		4373.01	1+	
4293.45	1+		4381.70	1+	
4294.86	1−		4382.92	½	Hazy
4296.98	1+		4384.74	1−	
4298.55	1−		4390		Very indistinct
4300.92	1+		4390.85	1+	
4302.63	1+		4400.30	1+	
4305.24	1+		4405.04	1−	
4307.07	1+		4410.12	1+	
4309.98	1		4413.09	½	
4311.89	1+		4420.42	1+	
4315.12	1		4421.59	1−	
4317.07	2	Slight shading toward blue	4430.94	1	
4320.68	1−		4436.48	1	
4322.74	1		4447.18	½	
4324.59	1+		4453.30	1	
4326.61	1		4458.03	1	
4328.77	1+		4465.01	1	
4331.38	½	Hazy	4477.15	1−	

Aluminium.—The aluminium arc in hydrogen gives a beautiful fluting with first head at λ 4241.26 and running toward longer wave-lengths. This fluting appears equally well in an atmosphere of ammonia (Plate I, Fig. 2). The following table gives the wave-lengths and intensities of the principal lines:

Wave-length	Intensity	Remarks	Wave-length	Intensity	Remarks
4241.26	3	1st head	4292.01	2	
4241.75	3		4294.31	3	
4242.41	2		4296.99	2	
4243.10	2		4298.10	3	
4243.94	3	Wide	4302.08	3	
4245.32	4		4302.65	1	
4246.58	3		4306.34	3	
4247.58	1		4310.82	3	
4248.09	2		4315.57	3	
4249.68	2		4320.63	3	
4250.34	1		4326.00	5	Impurity superposed
4251.44	2		4331.91	2	

Wave-length	Intensity	Remarks	Wave-length	Intensity	Remarks
4253.26	2		4338.37	2	
4255.22	2		4345.34	1	
4257.35	1+		4353.38	2	4th head
4259.71	3	Wide, 2d head	4354.13	1	
4261.18	3		4355.17	1	
4261.77	3		4356.64	1	
4262.59	3		4361.30	1	
4263.50	3		4362.21	1	
4264.58	3		4363.30	1	
4265.80	3		4365.18	2	
4267.24	3		4367.21	2	
4268.86	3		4368.	½	
4270.68	3		4369.67	2	
4272.72	3		4371.49	½	
4274.98	5	Impurity here	4372.54	1	
4277.70	4	Impurity here	4375.18	½	
4280.67	4		4375.97	1	
4283.94	4		4379.19	½	
4287.30	2	3d head?	4379.90	½	
4287.75	3		4388.23	1	
4289.91	3		4393.42	1	
4290.68	2		4399.19	1	

Magnesium.—The magnesium arc in hydrogen gives the three flutings discovered by Liveing and Dewar[1] in the magnesium-hydrogen spark, with first heads at λ 5618, 5210, and 4849 and running toward the *shorter* wave-lengths. The fluting at 5210, which is the one showing the plainest on my photographs is made up of such very fine lines near the heads that the principal head appears like a line by itself; but farther away from the heads the lines seem to become stronger and to overlap one another so that many of these lines are much stronger than the head itself and their distribution seems quite irregular. I mention this more particularly because it is characteristic of the hydrogen-zinc and hydrogen-sodium flutings described below. I have noticed that in the spark, the intensity of the magnesium flutings is greatly increased with respect to that of the *b* group by the introduction of inductance in series with the capacity shunted about the induction coil. The deposit in the hood enclosing the magnesium arc in hydrogen is quite plentiful, has a dark slate color, decomposes water at ordinary temperature, giving alkaline reaction, and oxidizes rapidly on heated platinum.

[1] *Proc. Roy. Soc.*, **32**, 189, 1881.

Zinc.—The zinc arc in hydrogen gives a collection of lines between about λ 4300 and 4050, having an average intensity from 2 to 4, and not found in the arc in air. This appears to be a set of flutings of complicated structure having heads less distinctly marked than usual and running toward the shorter wave-lengths. The semi-opaque deposit formed in the atmosphere of the hood is so considerable that a current of not more than about four amperes can be used. This deposit is dark brown in color, gives alkaline reaction in water, but does not decompose it enough to form bubbles even when heated. It dissolves completely in sulphuric acid, forming a clear solution, and rapidly oxidizes on heated platinum.

Sodium.—The sodium spectrum was obtained by using metallic sodium as the cooler rotating electrode, and copper as the stationary one. As above mentioned, there is not the slightest trace of any of the hydrogen lines to be detected in this spectrum, either visually or on the photographs, but there is a strong series of lines, between λ 5000 and 3800, resembling the magnesium-hydrogen series in character. This is probably a complicated fluting of heads less clearly marked than usual and running toward the shorter wave-lengths. A compound of sodium and hydrogen is already well known. The formation of the semi-opaque deposit in the atmosphere of the hood is so considerable that the arc can be run only about five minutes at a time. I have not tried the sodium arc in air.

The sodium spectrum obtained in hydrogen is itself quite interesting. All the sodium lines given by Kayser and Runge[1] come out very clearly, but the principal interest centers about the D lines, which are very intense, and so wide as to cover all the region between them. When observed visually their reversals change in width quite rapidly. At first these reversals may be quite narrow black lines, and then they quickly widen and blot out the whole of the bright field between them. The width of the two lines taken together is about 150 Ångström units, though the photographic plates are stained for a much

[1] KAYSER and RUNGE, *Wied. Ann.*, 41, 302, 1890.

greater width. The strongest copper lines show only very faintly, the weaker ones not at all.

Correlation of Effects.— In the metals arranged in the order given above (tin, silver, copper, magnesium, aluminium, zinc, and sodium) the following relations roughly hold:

(1) The set of lines characteristic of the spectrum of each metal in an atmosphere of hydrogen is stronger than that of the *preceding* metal of the series; (2) the hydrogen lines appearing in the spectrum of the metallic arc of each metal are stronger than in that of the *succeeding* metal of the series; (3) the general working of the metallic arc is worse for the metals at the first of the series than for those at the end. Briefly stated, the intensities of the hydrogen lines coming out in the spectra of various metals are roughly inversely proportional to the intensities of the characteristic flutings of those metals.

GENERAL EFFECTS OF THE HYDROGEN ATMOSPHERE.

Historical.—Liveing and Dewar[1] found the carbon arc to work badly in hydrogen, and to give spectral lines of different relative intensities than in air. Professor Crew[2] has given quantitative measurements of these changes of intensities for the metallic arc spectra of magnesium, zinc, and iron.

The general effects of the hydrogen atmosphere may be summarized thus:

(1) The arc works poorly in hydrogen. (2) The intensity of the whole spectrum is greatly reduced in hydrogen. (3) Those metallic lines which belong to the series of Kayser and Runge are uniformly reduced in intensity. (4) Other lines are reduced in intensity but not uniformly. (5) Certain lines supposed to belong to the spark spectrum make their appearance in the arc in hydrogen.

Discussion.—The radiations of the electric arc are generally admitted to be due to three causes: electrical, chemical, and thermal. The chemical cause must depend upon the electrical cause in some way, for the chemical cause cannot originate the

[1] *Proc. Roy. Soc.*, **33**, 430, 1882. [2] *Phil. Mag.*, **50**, 497, 1900.

arc, and the chemical cause follows the electrical in point of time, as is shown by the "luminous cloud" of Crew and Basquin above referred to. The thermal cause also must depend upon the electrical cause in some way. It probably depends upon it directly, but, in any event, it is a function of it through the chemical cause, for all chemical reactions either take in heat or give off heat.

Let us consider two arcs which are alike except that a larger current runs through the first than through the second. Since the secondary causes of radiation go hand in hand with the electrical cause, we may expect the first arc to have a spectrum which is uniformly brighter from one end to the other than that of the second arc. With the exception of a slight variation, probably due to conduction losses, this is just what is always observed, and confirms the secondary character of the chemical and thermal causes of radiation. If these causes were not dependent upon the electrical causes, we might possibly get an arc which would give only a flame spectrum, or an arc which would give only a spark spectrum.

Let us now suppose that we run the same current through both the similar arcs, and suppose that in some way we reduce the chemical action going on in the second arc. What difference may we expect to observe in them?

A reduction of the chemical action necessarily involves a reduction of the temperature of the arc, because the chemical reaction in the arc in air is exothermic. We have then an arc of lower temperature. If it is a stationary arc it will be shorter and will go out more frequently. If it is rotating it will have a smaller flame and work more poorly. All of which is amply verified by experiments in hydrogen.

But we may expect this reduction of chemical action to have certain effects upon the spectrum. If all the lines of the spectrum of this arc were functions of the *electrical cause alone*, then there would be *no reduction* in intensity of any part of the spectrum when the chemical action is reduced. Professor Crew estimates from five to one hundred times as the reduction in

intensity caused by the hydrogen atmosphere. The electrical cause alone can account, then, for only a small part of the radiation. The secondary causes play very important parts.

If all the lines of the spectrum of this arc were the *same function* of the causes of radiation, then all the lines of the spectrum would be *uniformly* reduced in intensity upon the reduction of chemical action. Experiment shows this hypothesis to be too broad, but the lines belonging to the series of Kayser and Runge are uniformly reduced in intensity, so that it is *probable* that these lines are all the same function of the causes of radiation.

Of the other lines those which are reduced more in intensity than the series lines must be less intimately related to the electrical or thermal causes of radiation than are the series lines.

Let us agree that the average intensity of the spectrum of the arc in hydrogen is only one fifth of its intensity in air, and let us agree that the electrical cause of radiation remains practically constant with constant current and voltage although the general intensity of the arc is greatly reduced by the hydrogen atmosphere, then it follows that of the total radiation, that fraction which must be attributed to the electrical cause alone, is relatively five times as great in hydrogen as it is in air. Any line, therefore, which is a function of the electrical cause alone, should have in hydrogen five times the relative intensity that it has in air. It seems quite likely that this may account for the appearance in hydrogen of numerous strong spark lines, not found in the arc in air.

The appearance of the spark lines in hydrogen is not confined to the rotating arc; the magnesium spark line at λ 4481 appears clearly in the stationary metallic arc in hydrogen but not in air. The above explanation for the appearance of these lines makes it probable that the electrical cause of radiation is not zero in either atmosphere.

In the rotating arc the current is interrupted about twenty-five times per second when the rotating electrode is a rod, instead of a disk, of metal, and this spark at the breaking of the current may account, in part, for the appearance of these spark lines in

hydrogen. But we may inquire why this spark should partake any more of the nature of the true spark in hydrogen than in air? The reduction of the chemical action in the arc reduces the temperature aud conductivity of the gases between the poles in hydrogen and it occurred to me that this action may affect the appearance of the spark lines in either of two ways:

1. It may be that a gas which is in the hot condition of the arc in air cannot give off spark lines; the arc spectrum may be characteristic of this condition of the gas and may have nothing to do with electrical action and so, in this state, would give off only arc lines if a spark were passed through it.

2. It may be that the conductivity of the gases in air is reduced so slowly at the breaking of the current in the rotating arc that the voltage of break never rises high enough to make a true spark.

In either of these cases, in hydrogen, the hot gases are largely absent owing to reduction of chemical action and give opportunity for the spark to appear.

In order to test the first suggestion I arranged an electrical circuit as shown in the diagram. The dynamo furnishes a direct current of 110 volts and when the switch was closed the current simply passed through the arc and the resistance in series.

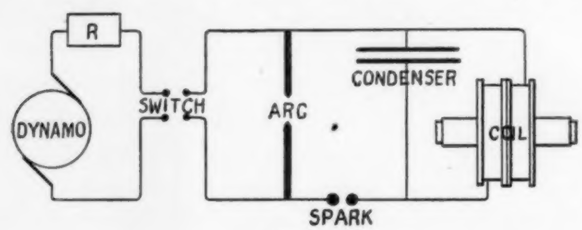


FIG. 1.

The arc was stationary, one electrode was carbon and the other a zinc rod. The induction coil used is a duplicate of the one designed by Professor Rowland to give a short spark but a very powerful discharge; an alternating current of 110 volts, 6 amperes, was run through the primary, without an interpreter. The condenser used has a capacity of $\frac{1}{80}$ microfarad. It will be noticed that the spark can take place only by passing in succession the two gaps marked "arc" and "spark." The

spectroscope is adjusted to observe the phenomena taking place at the gap marked "arc."

In performing this experiment I first turned on the spark and set the cross-hairs of the eyepiece of the 10-foot concave grating upon the zinc spark line at λ 5895, between the D lines of sodium. The spark was turned off and arc turned on. The spark line no longer appeared but came out instantly when the spark was again started along with the arc; both arc and spark were now running through the gap marked "arc" and the spectroscope showed both arc and spark lines. Now while both currents were on, the arc current was turned off; the arc spectrum disappeared but the spark spectrum persisted with apparently the same intensity as before and without any interval of darkness.

This experiment shows that the first suggestion is not true; that the arc spectrum is not characteristic of the condition of the gases in the arc, and makes it highly probable that the electrical cause of radiation is not zero.

In order to test my second suggestion above, I short circuited the spark gap shown in Fig. 1. The spark lines appeared as before in the spark, but disappeared as soon as the arc circuit was made; the arc and the spark discharges were both passing through the arc as before; I had simply cut out the "spark" gap, but the spark lines could not be seen when both currents were on. Now, when both currents were on, I broke the arc circuit, and nothing at all could be seen in the spectroscope; neither the arc nor the spark lines remained, although the spark current was still passing. After remaining at the eyepiece of the spectroscope about one second I began to see traces of the spark lines and then they soon came out with their usual brightness, and the spark discharge, which had been silent during that second of darkness assumed its usual noisy character.

This experiment shows that the gases of the arc do not furnish enough resistance to the passage of a high voltage alternating current to cause the discharge to assume the character of

a spark for a full second after the breaking of the arc current. This seems to confirm the second suggestion of page 14, to the effect that the conductivity of the gases decreases so slowly in the breaking of the arc current in air as to give rise to no very high voltage, and so accounts for the non-appearance of the spark lines in the rotating arc in air.

These two experiments throw an interesting light upon the nature of the spark. The spark at the arc gap in these experiments seems to be due to neither the current nor to the voltage, but to some kind of an impulse furnished by the sudden rush of electricity across the auxiliary "spark" gap.

In the second experiment above described, the spark lines do not all seem to come out at the same time. I hope in the near future to be able to arrange an automatic apparatus for making and breaking the currents and an adjustable occulting-screen which will enable one to photograph the spectrum of the spark at definite intervals of time after the arc current is broken. A series of these photographs will probably furnish an interesting story of the development of the spark spectrum.

ON A POSSIBLE FUNCTION OF DISRUPTIVE APPROACH IN THE FORMATION OF METEORITES, COMETS, AND NEBULAE.[1]

By T. C. Chamberlin.

According to a familiar doctrine founded on the researches of Roche, Maxwell, and others, a small body passing within a certain distance (the Roche limit) of a larger dense body will be torn into fragments by differential attraction. In reality, the doctrine is applicable to the close approach of any two bodies of sufficient mass and density, but, as this more familiar case of a small body in close approach to a larger body is the one supposed to be involved in the origin of comets and certain meteorites, it will at first be taken as representative, and the wider application of the doctrine will be considered later.

The sphere defined by Roche's limit is computed on the basis of a liquid body whose cohesion is negligible, and whose self-gravitation alone is considered. It is obvious, therefore, that when cohesion is a notable factor, a small body might pass through the outermost part of this Roche sphere without suffering disruption, but that, if a nearer approach were made to the large body, fragmentation might take place. There is, therefore, a sphere within the Roche limit—which may be called the sphere of disruption—which is applicable to solid bodies as distinguished from liquid bodies.

The size of this sphere of disruption compared with the Roche sphere depends, among other things, on the coefficient of cohesion and the size of the body to be disrupted. The coefficient of cohesion being the same, the sphere of disruption is relatively smallest when small bodies are to be disrupted, and becomes larger as the size of the body increases until it is

[1] I am greatly indebted to Dr. F. R. Moulton for suggestions and criticisms, and for formulæ for certain auxiliary computations that do not appear in the paper. I am under obligations to Mr. C. E. Siebenthal for the diagrams and other aid.

sensibly as large as the Roche sphere. To illustrate this concretely, let disruptions be supposed to take place along a diametrical section normal to the gravitative pull, dividing the body into halves. Let the bodies to be disrupted be spherical and homogeneous. The cohesion to be overcome will then obviously vary as the areas of the diametrical sections, and these areas vary as the squares of the radii of the bodies. But the masses of homogeneous spheres vary as the cubes of their radii, and the gravitative pull varies as the masses, modified by the differential tidal pull. It follows that mutual gravitation will more effectively disrupt large bodies than small ones. The limit at which the fragmentation of a solid body will take place will therefore approach more and more closely that of a fluid body as the size of the solid body becomes larger. For solid bodies of considerable dimensions, as asteroids, for example, the limit of disruption approaches sufficiently near Roche's limit to make the difference negligible in a general discussion. This will appear the more evident from the following numerical considerations.

Experimental data as to the tensile strength of rock are very limited, as the material is rarely used where tensile stresses are involved, but all the results of experimental tests given in Johnson's *Material of Construction* fall notably below 1000 pounds to the square inch, and this figure may be assumed as a liberal representative estimate. The weight of representative rock may be taken as $\frac{1}{10}$ pound per cubic inch. The tensile strength of an inch cube is therefore to its weight, at the surface of the Earth, as 10,000 to 1. Using the same data, the tensile strength of a mile-cube of rock is to its weight as 1 to 6.36, while that of a 100-mile-cube is as 1 to 636. It will be seen, therefore, that in a comparatively small body the cohesive resistance to disruption bears a very small relation to the gravity of the mass, and that for large bodies it is negligible. For such bodies, the Roche limit may be taken as appreciably the limit of the sphere of disruption.

These numerical considerations, however, show that fragmentation by differential gravity acting alone will not become

minute in any such case as that of a satellite or asteroid making a near approach to one of the planets.

But there are additional considerations that influence the practical result. The outer portion of the Earth, and doubtless that of the satellites, asteroids, and cold planets generally, is deeply traversed by fissures—oblique and horizontal as well as vertical—which render it little more than a pavement of dissevered blocks which could be lifted away with little resistance beyond that of gravity. The relief of pressure upon the less fissured portion below, which would follow upon the removal of the overlying fissured portion, and the sudden exposure of this under portion to a lower temperature resultant from this removal, would develop new stresses; and these would doubtless give rise to additional fissuring and further easy removal, and thus the process would be extended. It is not improbable that the sudden rending open of a sphere that is hot within and the consequent exposure of the highly heated rocks in the interior to much lower temperatures would result in sufficiently great differential contraction to minutely disrupt the fragments irrespective of differential gravitation. The central portions of a body sufficiently hot to melt at surface pressures would doubtless pass immediately into the liquid condition on the removal of the pressure of the overlying rock, and this passage might, not unlikely, take on eruptive violence by reason of the included and highly compressed gases—or substances in a potentially gaseous state—in which case an extremely minute division would ensue. In the case of the Earth, there is good reason to believe that if its interior gravitative stresses were suddenly removed, its internal elasticity would disrupt its exterior with much violence; and if the gravitative stresses were more gradually removed, the disruption would still be complete and pervasive, though less violent. How far a similar view may be entertained with reference to small bodies like the asteroids is uncertain, but even in these it is not improbable that the internal elastic factors would offset in some large part, if not entirely, the restraining force of the general cohesion of the mass.

From these considerations it would seem that the sphere of disruption, even in solid bodies of the nature of satellites and asteroids, may closely approximate to the theoretical Roche limit, while, for large bodies intensely compressed and very hot within, the practical sphere of disruption might actually exceed the Roche sphere. In the case of large gaseous bodies like the Sun, intensely heated and compressed in the central portions, the disruptive or dispersive sphere must be much larger than the Roche sphere. But of this later. For the smaller solid bodies, and for present purposes, it may be assumed that the sphere of disruption is practically defined by Roche's limit.

The size of the sphere of disruption compared with the size of the body producing the disruption is an essential point in this discussion. The relative magnitude of these varies for every couplet of bodies brought under consideration, because it is dependent on density, cohesion, internal elasticity, and other varying factors. Roche has shown that, if the two bodies are incompressible fluids of the same density, and without cohesion, the limit of disruption is 2.44 times the radius of the body producing the disruption. The cross section of this body will therefore be to the cross section of the Roche sphere as 1 is to 5.95. The disk of the outer ring of *Saturn*, compared with that of the planet, whose density is unusually low, is a trifle below this ratio (1:5.29), but may be taken as a practical sanction of the figure theoretically deduced. The disk of the Earth, a dense body, is to the disk of the Roche limit, as computed by Darwin, as 1 to 7.5. It may therefore be concluded that where planets and planet-like bodies are concerned, the sphere of disruption has a cross section from 5 to 7.5 times as great as the central body. It follows from this, that to a passing body the sphere of disruption exposes a disk five to seven times as great as the central body, and hence there are from four to six times as many chances that the passing body will invade the sphere of disruption without collision, as that it will strike the central body. In other words, *the fragmentation of a small body by near approach to*

a large one of the nature of the planets will be from four to six times as imminent as actual collision.

That disruptions or explosions of some kind actually take place in the heavens, and that not uncommonly, seems to be implied by the sudden appearance of new stars, often with great brilliancy, followed by rapid decline to obscurity or extinction.[1] Five such new stars have been recorded during the last decade, and the survey of the heavens during this period has not been entirely exhaustive. The appearance of such new stars has been referred to collision, but their frequency has been felt to be an objection to this view, and other explanations, of the nature of eruptions or explosions, have been offered, but usually without assigning any probable cause for such extraordinary explosive action. The numerical objection is, in some measure, removed if the possibilities of disruptive approach be added to those of collision; and it will be seen further on that special conditions giving rise to distant approaches that are merely disturbing at the outset, may ultimately give rise to large possibilities for disruptive approaches.

That bodies pass within the disruptive sphere of other bodies is known from the fact that at least four comets have been observed to pass within the Roche limit of the Sun, and these would quite certainly have been torn into fragments if they had not already been in that condition. There are, therefore, some observational grounds for the view that instances of bodies passing through the disruptive spheres of other bodies are not so rare as to render their results unimportant.

In the considerations now set forth, there seems to be warrant for the proposition *that solid bodies may suffer fragmentation without actual collision with other bodies, and that the bodies so disrupted may constitute comets so long as the fragments remain clustered, and that when these fragments become dispersed, they may constitute one variety of meteorites.* Only the first part of the proposition is novel—if indeed that is—for the disintegration of comets

[1] A fact which has become very familiar and impressive, since this was written, by the appearance of *Nova Persei.*

into meteorites is an accepted doctrine. The characteristics of comets other than their fragmental structure will need to be considered, but this may best be taken up later.

The foregoing conclusion, as a purely ideal proposition, does not appear to need discussion, unless the fundamental deductions of Roche, Maxwell, and others are questioned. Nor does its application to the adventitious cases of wandering bodies permit definite discussion, for neither the nature nor the number of such bodies is known; nor is the likelihood of their close approach to other bodies capable of estimation. But, on the probable supposition that the stars are centers of systems like our Sun, there are hypothetical cases of approach of these systems to each other that by disturbance of the planetary orbits may lead on to disruptive approach of the individual bodies, and thus give effective application to the doctrine; and these invite consideration. It must be confessed that these cases, likewise, cannot be discussed with much satisfaction, since the movements of the assumed solar systems and their relations to each other are but very imperfectly known. Present data, however, warrant the assumption that the stars and their attendants are moving in various directions at various velocities, and that they are probably not controlled by any central body; nor do they probably follow concentric orbits so adjusted to each other as to forbid close approaches. The conception that the movements of the stars are somewhat analogous to those of the molecules of an exceedingly attenuated gas in an open space, actuated by the attraction of their common but dispersed mass, seems the most probable that can be entertained in the present state of knowledge. It may at least be made the basis for the assumptions necessary to further discuss the doctrine in hand.

Let two stars be assumed to be attended by secondaries like those of the Sun, and to pass each other near enough to initiate serious disturbances in the orbits of the planets and satellites of the two systems. It is not necessary that this disturbance shall be so great as to bring about a disruptive approach of any of these bodies at once, but merely that this shall be the ulterior

effect, which may be long delayed. The two systems need not necessarily invade each other's actual limit, that is, the two suns need not approach each other within the sum of the radii of the orbits of their outermost planets.[1] For example, in the ideal case of two solar systems, it is not necessary that the orbits of the two *Neptunes* shall actually cut each other. If the undisturbed orbits merely touch each other, or even closely approach each other, it seems clear that if *Neptune* be at the time coming toward the point of such ideal contact, or near approach, the attraction of the passing sun, together with *Neptune's* own momentum, will carry the planet far beyond the limit of its own ideal orbit into the sphere of dominant influence of the passing sun. At the same time, the paths of the inner orbits of both systems will be distorted in a quite irregular way, dependent on their various positions in their several orbits. The transfer of an outermost planet from one system to another under these conditions of general disturbance, or any other radical change in the orbits of the outer planets, will quite certainly lead on to other disturbances of orbit, some of which may sooner or later lead to disruptive approach, though the result of such a complication is beyond the reach of precise prediction.

A still more remote approach between two systems in which the only result is a pronounced elongation of the orbits of the two systems, may ultimately result in close approaches, for, if the orbit of any of the planets of the two systems be elongated so that its perihelion distance is less than the aphelion distance of the next inner planet, or its aphelion distance greater than the perihelion distance of the next outer planet, a disruptive approach, although it will not necessarily follow, because the planes may not coincide, and for other reasons, may result — if not at once, at least ultimately — as a consequence of the shiftings and modifications which such a disturbed condition involves. For example, it is obvious that by a favorable conjunction with a passing system whose sun is distant from *Neptune* considerably

[1] In the illustrative examples it is assumed for convenience that the planes of the systems are normal to the systems' lines of movement.

more than the radius of his orbit, there may be an elongation of the orbit of *Neptune* so as to make it cut one or more of the inner orbits, and that further modifications may arise out of these relations which will either increase or decrease the eccentricity. The principles applicable here are identical with those that have been found to produce radical modifications of the orbits of comets and that have been worked out by H. A. Newton and others.

To embrace the full possibilities of the case, it is therefore necessary to consider (1) the effects of systems passing each other at distances varying from those in which the outermost planets do not even cut each other's orbits, down to center-on-center collisions, and (2) to take account of the ulterior effects of disturbed orbits, as well as the immediate effects. This last is a consideration of no small importance in the qualitative as well as the quantitative application of the doctrine, for it distributes the effects over an indefinite period of time, and does not require their coincidence with the passage of the systems. The ulterior effects, so far as the disruption of secondaries is concerned, may apparently be much greater than the immediate effects. If this is not already clear, let a specific case be taken, as, for example, two solar systems passing each other so that their centers shall be 500,000,000 miles apart at nearest approach. If the planes of the systems are transverse to their paths, the ideal undisturbed orbits of the asteroids will touch, or closely approach, or slightly cut each other, as the individual case may be. The ideal orbits of the *Jupiters* will fall but little short of the passing sun, while the ideal orbits of *Saturn*, *Uranus*, and *Neptune* will fall outside the passing sun. While the precise results of such an event cannot be computed, it is quite certain that the secondary systems of the two suns will be most profoundly disturbed and the symmetrical and harmonious relations of the planetary orbits be utterly broken up. While even in this case the *immediate* contingency of a disruptive approach of one secondary to another may not be high, there will arise a *perpetuated series of contingencies*, the consequences of which will

apparently be immeasurably greater than those immediately incident to the disturbing action, and the end of this perpetuated series of contingencies can scarcely be foreseen. Assuming that the great planets will exercise the same kind of influence over the small planets and asteroids that pass near them that *Jupiter* does over comets, the range of possible contingencies involves, on the one hand, closer and closer approaches and even collisions with the Sun and with other planets, and, on the other hand, the development of extremely elliptical orbits that will carry the small bodies into the sphere of influence of some other system. How large a proportion of these theoretical possibilities will be realized in a given disturbed system, it is impossible to determine, for the problem is far beyond the power of mathematical analysis, but it seems at least probable that results of moment may ensue.

If we may judge from the solar system, the small bodies may be assumed to be at least fifty times as numerous as the large ones, while not improbably they are a hundred or several hundred times as numerous. Other things being equal, they should show the characteristic effects of the action under discussion with correspondingly greater frequency. But the other conditions intensify these effects. A small body may be disrupted by a large one, but not necessarily the reverse. So, too, a small body may be thrown into an erratic orbit, while the orbit of the large body may not be sensibly affected, as shown by the changes in the orbits of comets caused by *Jupiter*. By far the most common effect of the close approach of two star systems should therefore be the fragmentation of the small bodies by being caused to pass within the spheres of disruption of the large bodies. As previously indicated, *the contingency of acquiring at the same time highly erratic orbits is imminent, and these are specially subject to still further changes, and thus these fragmental clusters come to possess by the very circumstances of their birth the second characteristic of comets, as well as the first.*

Whether they would possess at the same time, or come at length to possess, the *third* characteristic of comets, the attenuated matter of which cometic tails are made, is not so clear,

since the nature of this matter and its condition are not yet fully known. The recent discoveries relative to the extreme ionization of matter and perhaps even its corpuscular dissociation, and the radio-activity of certain kinds of matter are at least very suggestive in this connection. Six of the elements reported by good authority as detected in meteorites, are known to possess, or to be habitually associated with, radio-active matter, viz., barium, bismuth, cerium, lead, titanium, and uranium. It is not very material here whether this radio-activity is really possessed by all these elements themselves, or simply by substances associated with them. If the coma and tails of comets are dependent on rare substances of a radio-active or extremely volatile nature, and hence permanently retensible only in the interior of bodies, it would be difficult to imagine conditions more favorable for setting them free in unusual volume than minute tidal disruption; particularly is this true if the retention of these substances is dependent on low temperature, as seems to be the case, since they are brought forth and driven away at a highly accelerated rate as the Sun is approached. This view seems also to be supported by the fact that comets which remain long in the vicinity of the Sun, as for example the short-period comets, lose their tails in a brief period.

If the attenuated cometic matter owes its essential peculiarities to electric states, these might perhaps be derivable from the revolutionary movements of the magnetic elements in the fragmental swarm, for by the hypothesis of tidal disruption the swarm should inherit a rotatory movement, and the fragments should contain both magnetic and magnetizable matter, variously associated with diamagnetic matter.

That short-period comets are subject to progressive disintegration, and that their scattered elements constitute one class of meteorites, is familiar doctrine. There seems no reason for withholding the conception from comets which have parabolic or hyperbolic orbits, for in certain cases such comets have shown signs of disruption and partial dispersion in their perihelion passages. To the dispersed elements of these comets of high

velocity is assigned (in part at least) such meteorites as come to Earth from diverse directions with velocities incompatible with an origin within the solar system.

It remains to consider whether the fragments derived from the disruption of an asteroid, satellite, or small planet through differential gravitative strain without collision, will satisfy the characteristics which meteorites display. Ample data for a judgment on this vital point will be found in the articles on the structure of meteorites in the first two numbers of the *Journal of Geology* for the current year, by Dr. Farrington, who, at my request, has kindly brought together in succinct and systematic form the essential characteristics of meteoric structure. A study of these characteristics will show that, while they embrace a great and very significant variety, they are all referable to the structures that are appropriate to small planets, while it is difficult to see how all of these characteristics can be found in derivatives from any of the alternative sources to which meteorites have been assigned, namely, volcanic action of the Moon or of the planets, explosive projection from the Sun, or individual aggregation in space. Some of the matter is fragmentary, implying surface conditions, while some of it is coarsely crystalline, implying deep-seated conditions. Some is volatile and combustible, implying the absence of high temperature throughout its whole antecedent history, while some as distinctly implies the presence of high temperature. In some meteorites the iron is segregated, while in others it is disseminated. Frequent brecciated structures imply fracturing and recementation. *Faulting and slickensides* demonstrate movements attributable to the parent body, but not to the meteorite itself. Veins imply internal transfer and redeposit of molecules. The absence of the oxidation of the iron accords with internal conditions, and also with the supposed absence of atmospheres from small planets, asteroids, and satellites. In short, every feature of the meteorites, save, of course, the external effects of fragmentation and of heating during their fall through the atmosphere, is assignable to small planetary bodies riven into fragments without great

heat and, by reason of this, retaining the varied structure attained in the parent body.

As previously indicated, the disruption of a body like the Earth, the main mass of which has a temperature much above the melting point of its substances at low pressures, and which is greatly compressed within by self-gravity, would doubtless cause it to burst forth into a luminous body with perhaps some dispersive violence. The progressive stages of distortion which take origin in simple tidal protuberances and grow to greater and greater degrees of deformation and crustal fissuring, until the final stage of disruption is reached, could scarcely fail to bring some parts of the ocean into contact with some parts of the heated interior, with inevitable Krakatoan consequences. Fragments of the crust under these conditions might possibly give origin to meteorites, but the probabilities of such fragments being projected beyond the 640,000 miles of the Earth's dominant influence, or beyond the similar spheres of influence of other massive planets, would not seem to be great; and, if realized, the fragments would doubtless be reduced to dust, as in the case of the Krakatoan explosion, and this state of minute division would exclude such meteorites from recognition except as vanishing shooting stars. The probabilities are that the matter of a disrupted Earth or a similar massive planet would be again assembled into a planetary body by its strong self-gravity. The phenomenon would therefore be that of a temporary star. Assuming considerable dispersion, it might be rather brilliant for a time, but would rapidly cool as the result of such dispersion, and soon sink into invisibility. In the case of such a body as *Jupiter*, accepting current doctrine as to his nature, the initial brilliancy must be much greater and the cooling to invisibility much more prolonged.[1] To phenomena of this class may perhaps be tentatively assigned certain of the temporary stars. Obviously these can only be such as had no prior visibility, and such as sink sooner or later again into invisibility. Whether

[1] Such a body as *Jupiter* might perhaps, under proper conditions, be dispersed in the same manner as a sun as sketched beyond.

this invisibility were due to the superficial cooling of the nucleus, or merely to a deep enshrouding of cooled vapor, would be immaterial.

It has already been indicated that a possible result of the serious disturbance of one solar system by the near transit of another would be a fall of some of the disturbed planets upon one of the suns. This also might be an ulterior rather than an immediate result, through the modifying effects of other planets, as well as the direct effects of the primary disturbance. Such a fall must be presumed to give rise to a notable increase of heat in the central body, as well as to mechanical effects, both of which would be conditioned by the mass and velocity of the secondary. An outburst of greater or less brilliancy must be presumed to be the result. The mechanical effects upon the Sun would probably involve great changes in temporary density and condition, as well as outshoots of hot gases in various directions at high velocities. The effects might thus coincide with the phenomena of that class of the temporary stars in which a luminous state precedes and follows the outburst, and in which varying densities or high velocities in opposite directions seem to attend the temporary brilliant stage.

The disruption of suns has been neglected thus far. While, under the terms of the hypothesis, the disruption of these bodies must be rarer events than the fragmentation of the more numerous small bodies, the results must be correspondingly more important, by reason of their magnitude and character.

It has already been observed, in passing, that the internal elasticity of large hot bodies under great self-gravitative compression may so far aid in disruption, by coöperation with the differential gravity of an adjacent body, as to cause dispersion even before the Roche limit is reached. In the case of very large bodies that are already gaseous, such as the Sun, this phenomenon gives rise to a special case of extreme interest. Under this special case, there arise a large variety of particular instances

due to the varying sizes, velocities, paths, rotations, and constitutions of the couplets of stars concerned, and also to the adventitious effects of their secondaries; but, for a simple illustrative case, let it be assumed that two bodies of equal masses and equal velocities are approaching each other on parabolic paths, and that at periastron they will pass through each other's spheres of disruption, or rather, spheres of dispersion. For convenience, let if be assumed that one of these bodies (*A*, Fig. 1) is gaseous, while the other (*B*) has already become so cold and solid as to act essentially as a unit, though disrupted. The history of the dispersion of the gaseous body may then be followed alone. Let the rate of rotation of the gaseous body (*A*) be relatively low, as in the case of our Sun. It may then be neglected in a general discussion, since as a dynamic factor it is trivial compared with the enormous energies of momentum and of elastic dispersion involved. This will appear clear in the outcome. Furthermore, the direction of rotation with reference to the parabolic paths might happen to be any one of an indefinite number, in many of which the effect would be inconsequential, even if the energy were large. In the close approach of these two bodies the two great dynamic factors of special interest are (1) the tidal distortion, and (2) the elastic expansion of the gaseous body.

While the two bodies are yet distant from each other, they must begin to take on elongation of the tidal type as the result of their mutual differential attraction, this elongation being aided by the high internal mobility and elasticity of the gaseous body. As the bodies approach periastron, this elongation must progress at an accelerated rate. At the moment of the entrance of the gaseous body *A* upon the Roche sphere of the body *B*, self-gravitation, in accordance with Roche's doctrine, will have been completely neutralized on the lines joining the centers of *A* and *B*, and its restraining influence upon the elasticity of the gaseous body on these lines will have been removed, while the gravitative constraint in the transverse section will be increased. The expansive energy of the compressed gaseous matter will therefore be left to exert its full projectile force in the direction

of the axis of elongation. While I am unable to offer a numerical estimate of the magnitude of this expansional energy in such a body as the Sun, it is certainly of a high order of magnitude. The speed at which prominences are projected from the Sun under present conditions closely approximates to the parabolic velocity with respect to the Sun, and this is accomplished in spite of gravitation and a resisting atmosphere.

The case in hand, therefore, starts with simple tidal elongation at a distance, and increases to an explosive maximum as the bodies approach periastron. This increase is at first gradual, but in the last stages of approach to periastron the acceleration is exceedingly rapid. In any attempt to follow the process in more detail, adhering to the recognized principles of tidal action, four particulars are of special moment: (1) the progressive elongation of the body, (2) the change in the direction of tidal distortion, (3) the lag of the line of maximum elongation behind the line of maximum attraction, and (4) the rotatory effects arising from the gravitation of B on the tidal protuberances of A, which in this case will be peculiarly effective because of the enormous distortion of A and the very close proximity of B at the critical stages. The principles from which these effects arise are thoroughly demonstrated and are familiar to all students of tidal phenomena, and it is only their special applications to this case that need be discussed. The rotatory effects are a little peculiar in that both of the tidally acting bodies are rapidly approaching each other, and developing extraordinarily powerful differential attractions, while at the same time they are swinging about their common center of gravity. Near periastron they may be regarded as performing a semi-revolution about each other. By the terms of the special case in hand, this semi-revolution must be performed in a very few hours. During these few hours the gaseous body (A) is undergoing elongation at a rate not much less than that represented by its full explosive competency. The rotational forces are diagrammatically illustrated in Fig. 1, in which the lag is merely estimated and the distortion of A is simplified while that of B is neglected.

It is assumed that the lag of the axis of elongation of *A* is such that the effective path of explosive projection will be directed to the rear of *B*. It must be noticed that if the center of *A* passes through the outer part of the Roche sphere of *B*, the

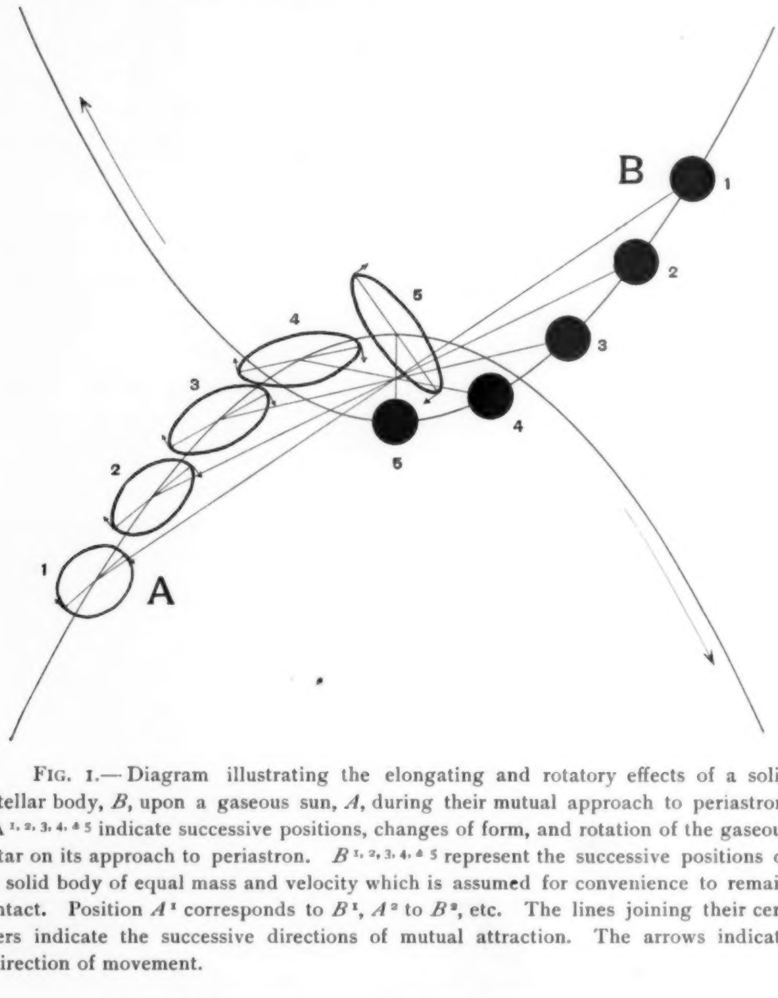


FIG. 1.—Diagram illustrating the elongating and rotatory effects of a solid stellar body, *B*, upon a gaseous sun, *A*, during their mutual approach to periastron. $A^{1, 2, 3, 4, 5}$ indicate successive positions, changes of form, and rotation of the gaseous star on its approach to periastron. $B^{1, 2, 3, 4, 5}$ represent the successive positions of a solid body of equal mass and velocity which is assumed for convenience to remain intact. Position A^1 corresponds to B^1, A^2 to B^2, etc. The lines joining their centers indicate the successive directions of mutual attraction. The arrows indicate direction of movement.

nearest edge of *A*, if undistorted, would pass within two or three hundred thousand miles of *B*, and hence that the projective elongation of *A* must pass critically near *B;* but the relative

speed of the bodies *A* and *B* is so great—both being near the parabolic velocity with respect to the other—that the projected matter of *A* can only collide with *B* on the supposition that the velocity of projection at least equals the parabolic velocity of the body and acts instantaneously, the last of which is impossible. This is based on the assumption that the transverse component of the attraction of *B* prevents the elongation of the minor axis of *A*, which is true of liquid bodies tidally affected, but might perhaps break down in a gaseous body under these extraordinary conditions. The point, however, is not important here, for if the edge of the projected part of *A* collide with *B*, it will only intensify the rotatory effects under consideration, and such collision is contemplated as an essential feature of the next following case, but is excluded here as the effects of *approach without collision* is the special theme under discussion.

The very close approach of the elongated extremity of *A* to *B* obviously gives great effectiveness to the rotatory influence of *B*'s attraction upon it. If the amount of this attraction be represented by the fall of $\frac{1}{1000}$ part of the mass of *A* toward *B* at a mean distance of 200,000 miles from *B*'s surface—the masses of *A* and *B* being each equal to that of the Sun—such a fall for about two hours and a half would generate a momentum equal to the whole revolutionary and rotatory momentum of the present solar system. It would appear, therefore, that under the conditions postulated a rotation of a highly effective kind must be imparted to the elongated body. It will now appear that the previous rotatory energy of the Sun, which is only about 2 per cent. that of the solar system, is a negligible factor.

The history of *A* then takes this form: (1) A very rapid elongation in the hour or two preceding its entrance upon the Roche sphere. (2) After entrance upon the Roche sphere, an explosive elongation actuated by the elastic energy then remaining in the body unrestrained by self-gravity in the axis of elongation. (A portion of the original elastic energy had been consumed in the previous elongation and a corresponding amount of momentum had been acquired, the larger component of which

would be effective along the changed line of elongation.) (3) After passing out of the Roche sphere, the restraints of gravity begin again to be felt and rapidly increase as *A* and *B* retire from each other, but the distance to which the extremities of *A* have already been projected, and the new relations thereby assumed to the remaining mass of *A*, and to *B*, render the renewed gravitative influence far less effective than the original, and the projection must continue until the momentum acquired is overcome. (4) Coincident with this projection a constantly increasing rotation toward *B* has been generated, which possibly reached an effectiveness comparable to that of the solar system. *The effects of explosive projection combined with concurrent rotation must obviously give rise to a spiral form.*

It seems clear from the nature of the case that there would be a certain brief period when the climax of projective effects would be reached, and that a stream of material of much greater mass and velocity than at other instants would at this time be projected from the extremities of the elongated mass in both directions. There should therefore be two chief arms to the resulting spiral starting from the opposite points of the central mass and extending outward to the limits of the spiral—indeed constituting the most outlying portions of the spiral. These must be curved in a common direction by the rotation of the mass. Such predominant arms are notable features in the typical spiral nebulæ. They are well shown in Nos. 1, 2, 3, 4, 5, and 6, Plate II, all of which are reproductions from photographs furnished by the late Professor Keeler.

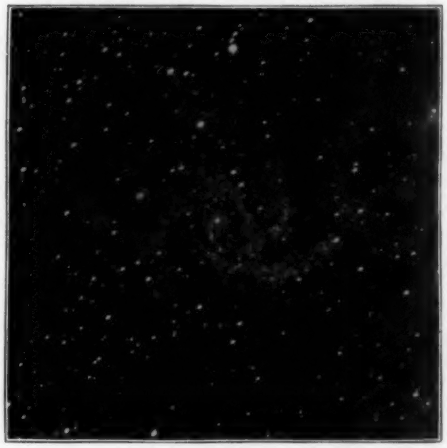
In the illustrative case that has just been discussed the solid body *B* was made to represent a convenient possible case but one whose real frequency is quite unknown, since extinct suns are beyond the reach of observation. If the active lives of suns are no greater than the periods deduced from computations founded on the Helmholtz theory of solar heat, extinct suns should either be numerous, or the whole previous history of the stellar system must have been short; or else, as a *tertium quid*, some effective means of regeneration must be assumed.

PLATE II

1

2

3



4

5

6

SPIRAL NEBULAE

1. *M 51 Can. Ven.=G. C. 3572--3574*
2. *M 101 Urs. Maj.=G. C. 3770—3771*
3. *M 74 Piscium=G. C. 372*
4. *H IV 76 Cephei=G. C. 4594*
5. *H 33 Trianguli=G. C. 352*
6. *H I 53 Pegasi* and her nebula

In the more typical case of two live suns coming into such close relations, its seems probable that mutual dispersion might follow without serious collision, since the analysis of the phenomena seems to show that the mutual elongations of the live suns would develop on essentially parallel lines whose con-

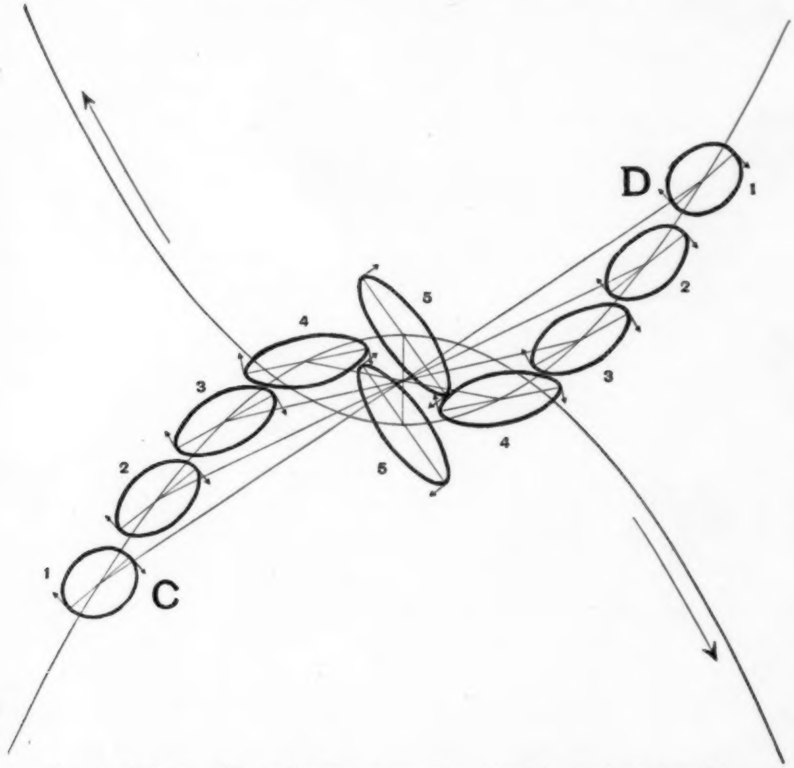


FIG. 2.—Diagram illustrating the progressive elongation and rotation of two suns, C and D, approaching perihelion. The position C^1 corresponds to D^1, C^2 to D^2, etc.; the lines joining these indicate the successive directions of mutual gravitation, and the arrows indicate direction of movement. The progressive elongation, the lag, and the rotation of the bodies at successive stages are diagrammatically indicated.

stant shiftings would be mutually consonant, as illustrated in Fig. 2. If no serious contacts were developed, the two resulting spirals would separate and pursue the paths normal to their parent stars, with such modifications as may have resulted

from the loss of energy involved in giving rotation to the nebulæ.

If, however, the periastron approach is so close that partial collision ensues, the analysis seems to indicate that the elongated

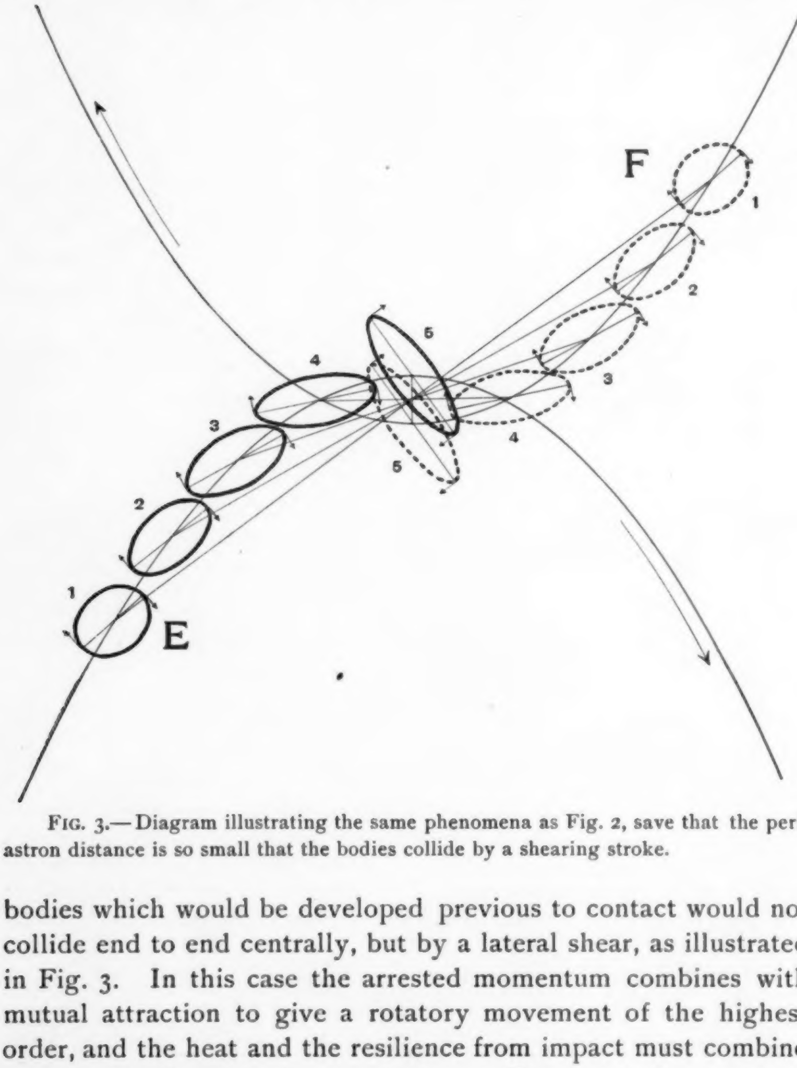


FIG. 3.—Diagram illustrating the same phenomena as Fig. 2, save that the periastron distance is so small that the bodies collide by a shearing stroke.

bodies which would be developed previous to contact would not collide end to end centrally, but by a lateral shear, as illustrated in Fig. 3. In this case the arrested momentum combines with mutual attraction to give a rotatory movement of the highest order, and the heat and the resilience from impact must combine to intensify the dispersive competency. The arrest of momentum

may be presumed to go so far in some cases as to cause the two bodies to unite to form a single spiral nebula of the largest and most dispersed order, such perhaps as the well-known great spiral nebulæ; or the arrest may be partial, and certain parts of one or the other, or both, of the masses may escape. In No. 1, Plate II, we seem to have a possible example of this, in which the escaping, or partially escaping, mass [1] is still associated with the longest arm of the spiral.

In case the collision of two suns becomes essentially central, a general dispersion of the most violent sort may be inferred to follow, and this may find exemplification in the vast irregular nebulæ, which are in many cases more or less radiant, and in some cases consist of two irregular masses which perhaps represent the wrecked originals. The collision of dead suns in which disruption shortly preceded actual impact may also play a part in forming irregular nebulæ.

Speculation may perhaps go so far as to attribute ring nebulæ to the central penetration of a concentrated solid body through a gaseous mass.

It is as impossible as it is unnecessary to consider here the infinite variety of sub-cases which the hypothesis under consideration involves, but it seems advisable to note that the case of equal suns with equal velocities, which has been used in illustration, is not the most prevalent case; for inequality of mass and momentum is quite certainly the rule, rather than equality or sub-equality. Where one of the suns is much smaller than the other, the dispersive influence will be most largely felt by it, and so it seems probable that there may be a series of cases in which the minor members of the couplets are dispersed with different intensities into complete nebulæ while the major members only suffer varying degrees of eruptive action or partial conversion into nebulæ and so perhaps become stars with nebulous adjuncts or atmospheres. Under this conception small nebulæ should be much more numerous than large ones. If

[1] This is assumed to have been a dead sun because of the limited evidence of explosive action.

large hot planets, such as Jupiter is supposed to be, are potentially gaseous, and if by disturbing approaches of stellar systems such planets are thrown out of their allegiance to their primary suns and take on comet-like courses, they would be specially liable to disruption and dispersion into small nebulæ, and would augment the number of the latter.

Whether the existing stellar movements and the mutual attractions of the stars are such as to give any substantial ground for believing that close approach can be a *chief* agency in producing comets, meteorites, and nebulæ, can only be determined when some approximate knowledge of the dispersion, the masses, the velocities, and the paths of the stars is gained. If the stars be considered simply as so many scattered bodies flying through space in straight lines at computed rates, and all mutual attractions and systematic relations be ignored, the frequency of disturbing approaches would not seem to be great and the quantitative value of the doctrine here sketched would seem to be questionable. The solar system has certainly never been subjected to disturbing approach since its present organization. But the assumptions made are certainly not the true ones and may not be representative. Besides the mere hazard of flying bodies, the mutual attraction of two stars after they enter upon each other's spheres of dominant influence — and these are very large — increases notably the probabilities of a disturbing approach even in the case of stars moving in opposed directions, while in the case of stars moving in sub-parallel and gently converging paths at sub-equal velocities, it may apparently become a dominant factor. At the average computed distances of the stars from each other, their mutual attractions are very slight, and in the central portion of the stellar system, in which the Sun seems to be placed at present, the general attractions are probably nearly balanced. Two stars, therefore, whose speeds are sub-equal and whose paths gently converge, may be controlled almost freely by their mutual attractions after they come within the spheres of each other's dominant influence. Such stars under mutual control would describe paths relative to each other

similar to those assumed in the discussion. Their closeness of approach at periastron would be determined by the *relative differences* (not the total amounts) of their speeds and momenta. The principle of sub-parallel movements applies here and gives results quite at variance with those that obtain in cases of opposed movements, where the *relative sums* of the velocities and momenta are to be considered. The movements of the long-orbit comets seem to be concrete expressions of this principle, as their perihelia are largely clustered on the front side of the Sun, *i. e.*, the side toward which it is moving, and they make close approaches to it. Such star clusters as the *Pleiades*, the members of which seem to have proper movements nearly the same in amount and direction, are doubtless also expressions of the principle of sub-parallelism, and in their remarkable nebulosity they may at the same time illustrate the doctrine of disturbed secondaries leading on to dispersive action, a part of the product of which remains associated with the stars themselves, while a part is more or less widely scattered, as the terms of the doctrine require.

If our stellar system has a definite boundary and is a flattened spheroidal cluster or a discoid, and if the ideal paths of the stars are elongate orbits stretching from border to border across the heart of the cluster (except as diverted by close approaches), then the orbital speeds and momenta should be lowest on the outer surface, and the paths should there be most frequently sub-parallel, and hence the conditions for the close approach of two suns through their reciprocal attraction be there most favorable. Now, visible nebulæ are most frequent in the regions polar to the Milky Way, and they may be regarded as lying on the flat sides or outer border of the stellar discoid where these conditions of low orbital velocity and momenta and prevalent sub-parallelism are dominant, and thus the distribution of nebulæ and the doctrine of close approach seem to be, so far at least, brought into harmony.

It may be needless to remark that the general conception lying back of the doctrine of dispersion by close approach has

a complementary regenerative or reconstructive phase, which, taken with the dispersive phase, makes up a cyclic process. With the disruptive action there is correlated a reciprocal concentrative action, which is supposed to reproduce organized systems out of the wreckage of disrupted systems. The notion is further entertained that the two processes may be mutually self-adjustable, within the limits of general conditions, and thus may give a large degree of perpetuity to the existing phase of the stellar system.

University of Chicago,
June 1901.

THE SHIFT OF THE CADMIUM LINE OF WAVE-LENGTH 4800 DUE TO PRESSURE.

By William B. Huff.

Several years ago Humphreys and Mohler, working in this laboratory, carried out a research on the pressure shift of the lines of the arc spectrum. Their results indicated a linear relation between the pressure and the shift. From a study of the lines due to substances contained in the poles as impurities, they also concluded that the shift was independent of the amount of material present; that is, that it was a function of the total pressure to which the arc was subjected rather than of the density of the vapor of the substance whose lines were being studied. It has also been found by Mr. L. E. Jewell that at atmospheric pressure the spectrum lines show a shift due to the presence of a large amount of the given material in the arc.

At the beginning of the present year, Professor Ames suggested to the writer a further study of the pressure shift of metallic lines, using alloys of various compositions. The shifts for a given metal used as a constituent of an alloy could be compared with those obtained when the metal alone was used. The results could then be examined as to possible effect of the amount of metal present and also for the effect due to the second metal of the alloy.

The apparatus employed was essentially that used by Humphreys and Mohler. The arc was in a heavy iron cylinder. The 21-foot concave grating gave a dispersion of about 1 mm to the Ångström unit in the second order. The camera was supplied with a shutter, so arranged that the center of the plate could be exposed to the spectrum as obtained under pressure. A second exposure gave the lines, at atmospheric pressure, above and below the first set.

It was necessary first of all to study the changes in lines which are produced by varying the amount of material in the

arc. Ordinary carbon poles contained so much sodium that the D lines were strong and bright when light from near the negative pole was brought on the slit. They reversed and the width of the reversal increased as the light was taken from points nearer the positive pole. Under pressure, the arc passes only when the poles are close together. This short arc, at atmospheric pressure, gave the D lines black on otherwise uniformly bright continuous spectrum. Their edges were as sharp as in the solar spectrum. A position of the image of the arc on the slit could easily be found for which D_1 was bright while D_2 remained black. A large amount of a sodium salt added to the arc caused the lines to broaden, until for a long arc they came together, the edges being very hazy. For a short arc they were somewhat sharper, owing to the effect of the continuous spectrum.

This great broadening was obtained for lines from other metals, but in no other case was a clean, black line obtained on a uniform, bright background.

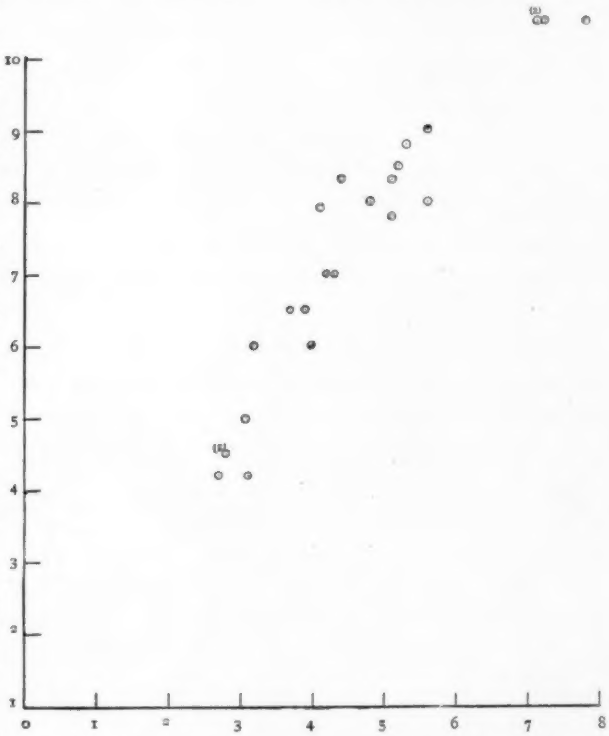
An asymmetrical broadening was often noted, *e. g.*, the potassium lines in the green. In the cases observed, anything like accurate settings to measure shift would have been difficult, even using the reversal as the center of the line.

Using cadmium in a carbon pole, it was found that for amounts sufficient to give the lines in the blue heavy and broad, these lines remained symmetrical and were not broadened toward the red. It was decided therefore to study the cadmium lines λ 4800 as obtained from pure cadmium and also from an alloy of cadmium and zinc and one of cadmium and lead; the first pair being much alike chemically as well as spectroscopically, and the second having nearly equal melting points. The alloys were made by fusing the metals together in a porcelain crucible, special care being taken to secure uniform composition.

When the alloy formed one pole of the arc, a large amount of vapor was formed, and this in a few seconds reduced the transmitted light to a dull, red, glow, entirely wanting in the shorter wave-lengths.

The best results were obtained by using carbon poles bored to a depth of about 2 cm and containing a small amount of alloy. It was found that 1 per cent. of cadmium in zinc gave the cadmium lines strong enough to photograph readily. They remained sharp, even under pressure, and the intensity was nearly uniform throughout the series of exposures, two at atmospheric pressure and an intermediate one for pressure shift. In the alloy with lead somewhat more cadmium was necessary.

The presence of considerable continuous spectrum was effective in keeping the edges of the lines sharp. Except for very short arcs, the metallic vapors cut out completely the carbon bands. This was particularly noticeable in the case of lead. A similar effect due to zirconium has been pointed out by Professor Rowland.



Ordinates: pressure; 1 unit = 1 atmos.
Abscissae: shift; 1 unit = 0.01 Å. U.

FIG. 1.

A large number of plates were taken before any of them were measured. The plates were taken in no special order. Needless to say, the greatest care was taken to prevent

accidental disturbances. Of the large number of plates taken, many were found to be faulty in several particulars and were rejected without being measured. None was thrown out merely because it showed an unexpected shift. The measurements were completed before any attempt was made to classify shifts or to plot curves. In all, more than forty plates were measured. The general results fall into two classes: shifts comparatively regular, though not increasing linearly with the pressure; and shifts which are decidedly erratic. For the first class a comparatively large amount of cadmium was used in the carbon pole. For the second group the cadmium in the arc was but a small part of the alloy present.

The tables give an idea of the order of agreement shown by the results. The pressures were indicated by a gauge, calibrated with a mercury column.

It is when the pressures and the corresponding shifts are plotted that the difference between the two sets of plates comes out most clearly. Cadmium alone, or in an alloy with but a small percentage of another metal, gives a series of points

PRESSURE SHIFT OF CADMIUM λ 4800.

Cadmium in arc.

Pressure.	Shift $\times 10^2$	Pressure.	Shift $\times 10^2$
4.5 atmos.	2.8 Å. U.	7.9 atmos.	4.1 Å. U.
4.5	2.8	7.8	5.1
4.9	3.1	8.3	4.4
6.5	3.9	8.3	5.1
6.5	3.7	8.8	5.3
7.0	4.2	9.0	5.6
7.0	4.3		

Alloy of cadmium + not more than 2 % zinc.

Pressure.	Shift $\times 10^2$	Pressure.	Shift $\times 10^2$
4.2 atmos.	3.1 Å. U.	8.5 atmos	5.2 Å. U.
4.2	2.7	10.5	7.1
6.0	4.0	10.5	7.1
6.0	3.2	10.5	7.2
8.0	5.6	10.5	7.8
8.0	4.8		

Alloy of zinc + 2% cadmium.

Pressure.	Shift × 10^2	Pressure.	Shift × 10^2
6.0 atmos.	3.0 Å. U.	8.5 atmos.	5.1 Å. U.
6.3	6.2	8.8	5.7
7.0	4.3	9.0	6.7
7.0	4.8	9.0	9.1
8.5	5.0		

2% cadmium in lead..

Pressure.	Shift × 10^2	Pressure.	Shift × 10^2
4.2 atmos.	1.0 Å. U.	6.0 atmos.	6.1 Å. U.
4.2	3.2	6.0	3.9
4.2	0.5	8.0	4.1
4.2	2.3	8.3	6.0
6.0	6.2	8.5	5.3

which are fairly regular and which seem to indicate that the relation between pressure and shift is nearly, but not exactly, a linear one (Fig. 1).

Weighting these values for the shift according to the reliability of the plate for measurement, and taking means for pressures approximately the same, the following ten points are obtained and used to determine a mean curve (Fig. 2).

No. Plates.	Pressure.	Shift × 10^2.
2	4.2 atmos.	2.8 Å. U.
2	4.5	2.8
1	5.0	3.1
2	6.0	3.6
2	6.5	3.8
5	7.9	4.7
2	8.3	4.9
1	8.5	5.2
1	8.8	5.3
4	10.5	7.2

This resultant curve does not pass exactly through the point (0,1) corresponding to zero shift at atmospheric pressure. This may indicate experimental errors, or it may be due to a shift arising from varying amounts of metal present in the arc. The possibility of error is too great to admit of any definite statement as to this question, since it is just here that accidental disturbances would most seriously affect results. Suffice it to say

that while the lower part of the curve is nearly a right line, with increasing pressure, the resulting shift becomes proportionally greater and the curve clearly becomes concave to the pressure axis.

The second group of plates shows the pressure shift when but little cadmium was used with a large amount of lead or of zinc. The pressures with corresponding shifts are shown in Fig. 3. It is impossible to determine any curve from the points laid down, yet the lines which gave these results are fully as clean and fine, and in all ways appear as reliable as do those of the first set. The results for the cadmium-lead alloy show a variation which seems attributable to the presence of the lead. The results for the cadmium-zinc show rather more regularity, but here, too, there seems to be more variation than cadmium alone shows.

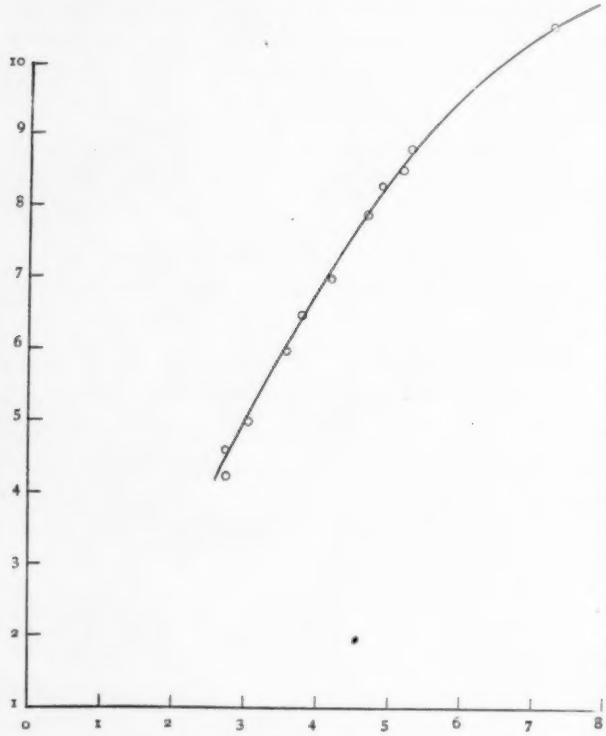


FIG. 2.

In work of this character accidental disturbances are very serious and effectually prevent more than approximate agreement in results. Giving a double exposure for the spectrum at ordinary pressure would enable one to detect an accidental

displacement of the camera only when sharp and fine lines are being studied. For heavier lines (all others are usually cut out by the metals present) a second exposure for only a part of the length of the ordinary line gives an effective test for accidental displacement.

Another possibility of serious error arose from the longer exposures necessary when but a small percentage of cadmium was used. It is difficult to say exactly how long the exposures were, but they usually lasted from half a minute to a minute. The accumulation of vapors gradually shut off the light, and the arc was continually going out. During such an exposure, the heating would raise the pressure as much as an atmosphere. The one recorded is the mean of the initial and final pressures. The fact that the metal was below the end of the carbon pole made it necessary to allow a little time for vaporization to begin before the pressure was noted.

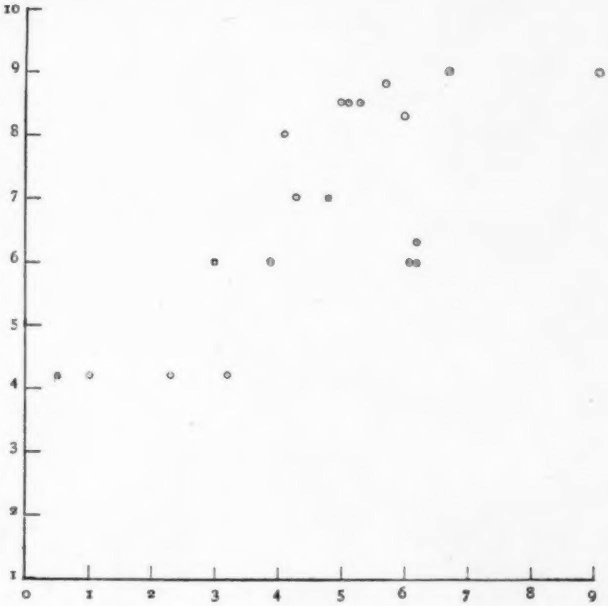


FIG. 3.

In the plates from the arc containing but a small percentage of cadmium a longer exposure was necessary, and the lines of the other metal of the alloy were usually strongly reversed. It was hoped to use zinc λ 4812 as a check on the cadmium shift, but the zinc line was so heavy that definite measurements on it

were impossible. It can be said, however, that the results of the measurements attempted differed quite independently of the cadmium lines.

The cadmium line was generally found not broadened. When the vaporization took place very rapidly, the line usually became hazy and broadened to the red. Despite the tendency to haziness and reversal shown by lines under pressure, many plates were obtained showing sharp, clean, lines. In no case are the measurements of a shift of a reversed line given. The question as to whether the shift of a sharp line is the same as that of one which is reversed seems not yet to have been settled.

The general results, therefore, from the two groups of plates go to show that the amount of a metal present does affect the pressure shift. The further fact that some of the best plates obtained were from the alloy of cadmium and lead and that these show wide variation indicates that the presence of a second metal is not without an effect.

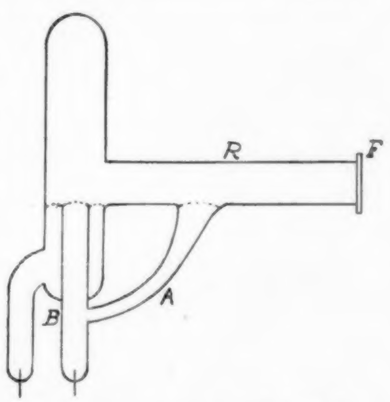
Finally, the relation of pressure and shift is not a linear one.

PHYSICAL LABORATORY,
JOHNS HOPKINS UNIVERSITY,
June 1901.

ON THE COMPOUND TRIPLETS IN THE LINE SPECTRUM OF MERCURY.

By C. Runge and F. Paschen.

In an article published in 1893, J. R. Rydberg[1] deals with the compound triplets of the first secondary series in the spectra of calcium, strontium, zink, cadmium, and mercury. These triplets do not merely consist of three single lines, but each of the three components is accompanied by weaker satellites whose distances follow certain laws that Rydberg has found. He then remarks that, as far as the observations of Kayser and Runge show, the compound triplets in the spectrum of mercury do not fall into the same order as the corresponding lines of the other elements, and he comes to the conclusion that Kayser and Runge's observations are probably incomplete and require revision. In an investigation, not yet completed, on the radiation of light in a magnetic field, the authors have studied the light of vacuum tubes with mercury electrodes[2] and the light of an Arons mercury lamp in a form not unlike the one used by Perot and Fabry. This lamp had the above arrangement:



It differs from Perot and Fabry's arrangement in the tube R, which is closed by a quartz window F and communicates with the tube B through the tube A. This tube serves to keep the level of the mercury in B near the mouth and at the same time

[1] *Wied. Ann.*, **50**, 625, 1893; and *Öfversigt af kongl. Vetenskaps Akademiens Förhandlingar*, 1893. No. 8, Stockholm.

[2] Of the form invented and described by F. Paschen. *Physikal. Zeitschrift*, **1**, 478, 1900.

to lead the mercury which condenses in *R* back to *B*. The lamps can be run for an hour without cooling.

On analyzing this light by means of a large Rowland grating we found, indeed, that the observations of Kayser and Runge have to be supplemented by a few lines of lesser intensity. The wave-lengths of these lines were either interpolated between the wave-lengths measured by Kayser and Runge or determined by means of one of the wave-lengths of these observers and the wave-lengths of its ghosts. A more accurate determination would have to take account of the fact that the lines here given as single probably all consist of several components. In the higher orders of the Rowland grating distinct traces of the components are to be seen; but the dispersion is not quite sufficient to measure them satisfactorily. With the supplementary lines found by the authors the two compound triplets observed consist of the following components:

λ	$\frac{1}{\lambda}$	λ Kayser & Runge	λ	$\frac{1}{\lambda}$	λ Kayser & Runge
3663.46	27296.60	3663.25	3027.66	33028.81	3027.62
3663.05 [1]	27299.65		3025 79	33049.22	....
3655.00	27359.78	3654.94	3023.64	33072.72	3023.71
3650.31	27394.93	3650.31	3021.68	33094.17	3021.64
3131.95	31928.99	3131.94	2655.29	37660.67	2655.29
3131.66	31931.95	3131.68	2653.86	32680.96	2653.89
3125.78	31992.01	3125.78	2652.22	37704.27	2652.20
2967.64	33696.81	....	2536.12	39430.31	....
2967.37	33699.87	2967.37	2534.89	39449.45	2534.89

Writing the wave numbers $\frac{1}{\lambda}$ in the way in which Rydberg shows the arrangement of the compound triplets, we obtain the table on opposite page.

The difference between the first and second column of wave numbers and the difference between the second and third column are in accordance with the corresponding differences in the

[1] This line has also been observed by W. B. HUFF, though not measured. ASTROPHYSICAL JOURNAL, **12**, 108, 1900.

triplets of the second secondary series, as far as the accuracy of the determinations goes. With the supplementary lines found by the authors the compound triplets in the spectrum of mercury closely resemble the compound triplets in the spectra of calcium, strontium, zinc, cadmium. There is, however, this difference, that in the spectrum of mercury there is one satellite more

Wave numbers	Difference	Wave numbers	Difference	Wave numbers
27,296.60	4632.39	31,928.99	1767,82	33,696.81
Diff.: 3.c5		Diff.: 2.96		Diff.: 3.06
27,299.65	4632.30	31,931.95	1767.92	33,699.87
Diff.: 60.13		Diff.: 60.06		
27,359.78	4632.23	31,992.01		
Diff.: 35.15				
27,394.93				
33,028.81	4631.86	37,660.67	1769.64	39,430.31
Diff.: 20.41		Diff.: 20.29		Diff.: 19.14
33,049.22	4631.74	37,680.96	1768.92	39,449.45
Diff.: 23.50		Diff.: 23.31		
33,072.72	4631.55	37,704.27		
Diff.: 21.45				
33,094.17				

to each main line. Rydberg suggests that there is in all compound triplets an infinite number of satellites to each main line, of which only a few are strong enough to be observed.

The relative intensities of the radiations whose wave numbers appear in a horizontal line in the table given above are by no means the same in the different horizontal lines. The radiation, for instance, corresponding to the wave number 27,299.65 is much weaker relatively to 31,931.95, than 27,296.60 to 31,928.99 or 27,359.78 to 31,992.01.

But the radiations corresponding to one another in the two compound triplets have the same relative intensities.

HANNOVER,
June 1901.

THE MOTION OF *POLARIS* IN THE LINE OF SIGHT.[1]

By J. HARTMANN.

AT the third conference of astronomers and astrophysicists, held at Williams Bay, Campbell made, on September 8, 1899, the interesting announcement that he had succeeded in spectroscopically resolving the Pole-star, *α Ursae Minoris*, into a system of at least three bodies. He published the data of observation which led to the discovery in the ASTROPHYSICAL JOURNAL (**10**, 180, 1899), and somewhat more extensively in the *Publications of the Astronomical Society of the Pacific* (**11**, 195, 1899). Campbell's observations showed that the visible star *α Ursae Minoris* has a variable motion in the line of sight, moving about the center of gravity common to itself and the invisible body in a period of $3^d\ 23^h\ 15^m$. The amplitude of this motion is very slight, the maximum velocity amounting to only ± 3 km. The motion of the center of gravity of this system is not, however, constant, but slowly variable, compelling the assumption of a third body. As the latter motion has a still unknown period of many years, I beg to designate it as the secular motion to distinguish it from the motion of short period.

At the time of this discovery the great photographic refractor of the Astrophysical Observatory had just been mounted, and the first plates with the new spectrograph (No. III) attached to it could be taken in February 1900. Since the short period variations in the velocity of *Polaris* are of such slight amount as to be demonstrable by only very accurate observations, this star seemed an excellent object for testing the efficiency of the new spectrograph; and this the more since the attempt had elsewhere been made in vain to confirm Campbell's valuable observations. It did, indeed, later appear that the selection of this star was

[1] Translated, at the author's request, from the *Sitzungsberichte der K. Akad. zu Berlin*. Session of April 18, 1901.

unsuitable in this respect—that a series of at least five successive days of observation were essential for a wholly independent determination of the period. Under the very unfavorable weather conditions prevailing in the spring of 1900 I only succeeded in obtaining four successive days of observation. They yielded the following velocities relative to the Sun:

1900	April 2.38	−13.8 km
	3.37	−17.7
	4.36	−15.3
	5.42	− 9.8

These observations displayed a variation of short period, and when three further successful observations were obtained on April 23, 24, and 25, which well fitted those above, on the assumption of a four-day period, it could then be said that a confirmation of Campbell's discovery had been attained.

It became apparent in the first observations made with the new spectrograph that the plates were affected in a very marked degree by the variations in the temperature of the air. In order to remedy this difficulty the whole spectroscope was enclosed in a box of light wood, within which the temperature of the air can be automatically kept constant. These alterations in the apparatus caused an interruption of the observations until autumn, which unfortunately again brought very unfavorable weather. It was not until January 1901 that I succeeded in getting the desired number of observations for an accurate determination of the velocity curve. As this series is distant by one and a half years from Campbell's measures I shall employ it in what follows for deducing a more accurate value of the length of the period. It will then be possible to correct all previous observations for the effect of the short-period motion and in this way to obtain the data for the determination of the secular motion.

A summary of my observations is given in Table I. In order to facilitate the comparison with the results of other observers I give the epochs of the plates in Gr. M. T., as well as in Julian days, the first three figures, 241, being in all cases omitted. As I have elsewhere shown, the velocities so far derived from the

plates are to be regarded as provisional on account of the lack of definitive values of the wave-lengths of the spectral lines employed. In a definitive reduction of the data, however, the individual results would be changed only by the fraction of a kilometer.

TABLE I.

Plate No.	Gr. M. T.					Julian day	V
			d	h	m	d	km
III 54	1900	Mar.	7	10	8	5086.42	−15.1
61			9	10	30	5088.44	−14.2
64			10	10	10	5089.42	−13.1
66			11	7	25	5090.31	−11.7
71			14	11	35	5093.48	−17.1
77			21	11	50	5100.49	−12.7
92		Apr.	2	9	0	5112.38	−13.8
94			3	8	50	5113.37	−17.7
101			4	8	40	5114.36	−15.3
102			5	10	10	5115.42	−9.8
130			23	9	40	5133.40	−16.4
134			24	10	50	5134.45	−13.6
135			25	8	42	5135.36	−4.7
228		Nov.	7	8	0	5331.33	−14.2
234			8	6	5	5332.25	−12.8
235			8	9	0	5332.38	−14.0
236			8	16	30	5332.69	−12.6
237			9	5	15	5333.22	−9.7
241		Dec.	3	9	35	5357.40	−9.6
254*	1901	Jan.	8	5	30	5393.23	−10.7
255			8	8	40	5393.36	−11.5
256			9	4	39	5394.19	−12.4
258*			9	8	56	5394.37	−13.9
266			10	4	12	5395.18	−17.3
269*			11	4	2	5396.17	−13.5
272			11	9	20	5396.39	−12.9
275			14	4	25	5399.18	−15.4
277*			14	8	55	5399.37	−15.9
278			15	4	20	5400.18	−12.6
280			16	4	25	5401.18	−9.5
284			16	9	35	5401.40	−11.2
289			17	4	25	5402.18	−12.0
290*			17	5	3	5402.21	−14.7
295*			17	10	33	5402.44	−15.3
296			18	8	30	5403.35	−15.1

* Plates thus designated were measured for me by Mr. Hansky, of Pulkowa, who is residing in Potsdam for the purpose of familiarizing himself with spectroscopic work. Plate III 290 was exposed for only ten minutes, as a test of the light-power of the 80 cm refractor. Although well measurable, it is still somewhat too faint. Hence the result derived from it will not be used further.

The observations from the 8th to the 18th of January, 1901, show the periodic change of V very clearly. I have platted

these in the accompanying cut, reduced to two successive revolutions, and have drawn the velocity curve, which shows great similarity to the one drawn by Campbell from his observations. Campbell also gives the descent from maximum to minimum as steeper than the ascent to maximum, although it is somewhat more strongly indicated in my measures than in his. A more extensive mass of data will be necessary to decide whether or not this change in the curve is real. The amplitude of the variation in velocity turns out to be 6.0 km, just what it was found to be by Campbell.

The following epochs may now be derived from the curve:

Maximum on 1901, January 12 = $5397\overset{d}{.}48$
Minimum on 1901, January 13 = 5398.97

My remaining dates of observations are so scattered that they can contribute nothing of importance to the curve, but they are sufficient for the sure determination of the whole number of revolutions elapsed.

The observations gave:

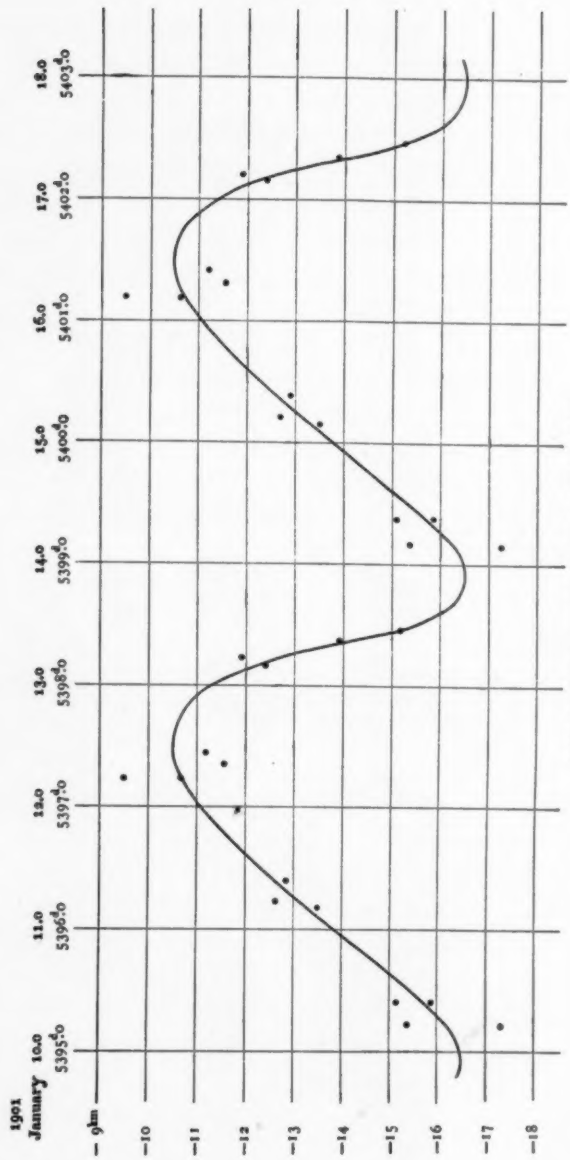
			Diff.
Maximum 1900	April 5,	$5115\overset{d}{.}42$	
			$217\overset{d}{.}80 = 55\ P$
	Nov. 9,	5333.22	
			$24.18 = 6\ P$
	Dec. 3,	5357.40	
			$40.08 = 10\ P$
1901	Jan. 12,	5397.48	
		282.06	$= 71\ P$

I have drawn a curve from Campbell's observations in 1899 which yields these epochs:

Maximum 1899 Aug. 30 = $4897\overset{d}{.}29$
Minimum 1899 Sept. 1 = 4899.06

The interval from August 30, 1899, to April 5, 1900, is 218 days, corresponding to 55 revolutions. Hence there must fall $71 + 55 = 126$ periods between August 30, 1899, and January 12, 1901, whence we derive the following period P:

Maxima	Minima
$5397\overset{d}{.}48$	$5398\overset{d}{.}97$
4897.29	4899.06
500.19	499.91
$P = 3\overset{d}{.}9698$	$P = 3\overset{d}{.}9675$



VELOCITY CURVE OF *POLARIS*

The small difference between the two values is due to the displacement of the relative position of the minima with the maxima as mentioned above. We may regard the mean of these two figures, viz.:

$$P = 3^{d}.9686,$$

as the true period. If we assume that the interval of 500 days employed is still uncertain by $0^{d}.1$, P will be accurately determined to one five-thousandth of its value, or to $0^{d}.0008$. The uncertainty does not amount to half a period, so that one could be in doubt as to the number of revolutions completed for more than 2500 revolutions or for 27 years. Campbell's observations in 1896, as well as those of Vogel in 1888, must therefore fit the curve with perfect accuracy.

Campbell's seven plates in 1896 are distributed so unfavorably that an independent velocity curve cannot be derived from them. If we apply to them Campbell's curve for 1899 these two epochs result:

Maximum 1896 Oct. 8 = $3841^{d}.77$
Minimum 1896 Oct. 10 = 3843.55.

266 revolutions are thus included between the epochs given for 1896 and 1899, hence $266 + 126 = 392$ revolutions between 1896 and 1901. Therefore we have for the definitive determination of P from the interval of 1896 to 1901:

Maxima	Minima
$5397^{d}.48$	$5398^{d}.97$
3841.77	3843.55
1555.71	1555.41
$P = 3^{d}.96865$	$P = 3^{d}.96791$

We may regard the mean of these two figures as the definitive value of the period, whence we get[1]

$$\mathbf{P = 3^{d}.9683 = 3^{d}\ 23^{h}\ 14^{m}\ 21^{s}}.$$

[1] On account of the secular motion of the system this period is not identical with the true period of revolution of the star. To determine the latter, we must apply to P a correction $\frac{PR}{L}$, where R is the secular velocity and L the velocity of light. This small correction cannot be computed until the secular motion has been accurately investigated, and I shall therefore not discuss this until a later time. For the computations given here we do not employ the true period of revolution, but the above apparent period, which holds good for a value of R of about -14 km per sec.

If we estimate the uncertainty of the last interval employed, 1555 days, as $\pm 0^{d}.25$, the uncertainty of P will be $\pm 0^{d}.0006$. This period is precisely equal to that of the well-known spectroscopic binary *β Aurigae*.

The observations of Vogel and Scheiner on the velocity of *α Ursae Minoris* are based on two plates taken on November 14 and December 6, 1888. These isolated observations can contribute nothing toward a more accurate determination of the period, but they are of great value in investigating the secular motion. To determine this, we must correct the values of V for the particular dates of observation for the effect of the periodic motion. I have tabulated in Table II the values of this correction, B, with the argument A, which is the interval in days elapsed since the last preceding minimum. These corrections have been taken from a curve which equally well represents Campbell's observations and my own.

TABLE II.

A	B	A	B	A	B
d	km	d	km	d	km
0.0	+3.0	1.4	−1.0	2.8	−2.2
0.1	+2.9	1.5	−1.3	2.9	−1.7
0.2	+2.8	1.6	−1.6	3.0	−1.2
0.3	+2.6	1.7	−1.9	3.1	−0.6
0.4	+2.3	1.8	−2.2	3.2	+0.2
0.5	+2.0	1.9	−2.4	3.3	+0.9
0.6	+1.7	2.0	−2.6	3.4	+1.6
0.7	+1.3	2.1	−2.8	3.5	+2.2
0.8	+1.0	2.2	−2.9	3.6	+2.5
0.9	+0.7	2.3	−3.0	3.7	+2.7
1.0	+0.4	2.4	−3.0	3.8	+2.9
1.1	+0.1	2.5	−2.9	3.9	+3.0
1.2	−0.3	2.6	−2.7	4.0	+3.0
1.3	−0.6	2.7	−2.5		

I employed as the initial epoch for computing the times of minima the mean of the two best-determined epochs, viz., Campbell's minimum on September 1, 1899, and mine on January 13, 1901. The initial epoch is thus obtained:

$$\text{Minimum} = 5149^{d}.01.$$

With these figures I will now correct for periodic motion the determinations of the velocity of *Polaris* so far published, in order to prepare the observed data for the investigation of the secular motion.

I. OBSERVERS: VOGEL AND SCHEINER.

(*Publ. des Astrophys. Obs. zu Potsdam*, Bd. VII, Theil I, p. 96.)

No.	Gr. M. T.	Julian	V	Minimum No.	Minimum Epoch	A	B	R
	h	d	km		d	d	km	km
1	1888 Nov. 14 6.1	0956.25	−23.5	1	0944.52	1.73	−2.0	−25.5
2	Dec. 6 8.2	0978.34	−28.2	7	0978.33	0.01	+3.0	−25.2

It is seen that after allowance for the periodic motion, the two observations, which are the mean of the measures of Vogel and Scheiner, come into surprisingly good agreement.

Mean: R = − 25.33 km, for 1888 Nov. 25.3 ($0967^{d}.3$).

II. OBSERVER: CAMPBELL.

(*Publ. of the Ast. Soc. of the Pacific*, 11, 195.)

No.	Gr. M. T.	Julian	V	Minimum No.	Minimum Epoch	A	B	R
	h	d	km		d	d	km	km
1	1896 Sept. 8 22.8	3811.95	−20.1	721	3811.69	0.26	+2.7	−17.4
2	15 22.8	3818.95	−19.1	722	3815.66	3.29	+0.8	−18.3
3	23 21.4	3826.89	−18.9	724	3823.60	3.29	+0.8	−18.1
4	Oct. 5 21.0	3838.88	−19.0	727	3835.50	3.38	+1.5	−17.5
5	Nov. 11 19.3	3875.80	−20.1	737	3875.18	0.62	+1.6	−18.5
6	12 (18.)	3876.75	−16.9	737	3875.18	1.57	−1.5	−18.4
7	Dec. 8 16.7	3902.70	−20.3	743	3898.99	3.71	+2.7	−17.6
8	1899 Aug. 9 0.8	4876.03	−13.0	989	4875.20	0.83	+0.9	−12.1
9	9 20.1	4876.84	−10.8	989	4875.20	1.64	−1.7	−12.5
10	14 22.8	4881.95	− 8.9	990	4879.17	2.78	−2.3	−11.2
11	16 0.1	4883.00	−13.9	990	4879.17	3.83	+2.9	−11.0
12	23 0.3	4890.01	−10.7	992	4887.10	2.91	−1.6	−12.3
13	24 0.8	4891.03	−15.0	992	4887.10	3.93	+3.0	−12.0
14	26 0.9	4893.04	− 9.0	992	4887.10	1.97	−2.5	−11.5
15	27 0.3	4894.01	−10.6	993	4891.07	2.94	−1.5	−12.1
16	27 16.2	4894.68	−14.0	993	4891.07	3.61	+2.5	−11.5
17	28 0.8	4895.03	−14.5	993	4891.07	3.96	+3.0	−11.5
18	28 16.3	4895.68	−13.7	993	4891.07	0.64	+1.5	−12.2
19	29 0.4	4896.02	−12.1	994	4895.04	0.98	+0.5	−11.6
20	29 18.8	4896.78	− 9.6	994	4895.04	1.74	−2.0	−11.6
21	30 0.0	4897.00	− 8.9	994	4895.04	1.96	−1.5	−11.4
22	30 16.2	4897.68	− 9.3	994	4895.04	2.64	−2.6	−11.9
23	Sept. 4 16.2	4902.68	−14.1	995	4899.01	3.67	+2.6	−11.5
24	6 18.1	4904.75	− 9.2	996	4902.98	1.77	−2.1	−11.3
25	11 16.2	4909.68	− 9.4	997	4906.94	2.74	−2.4	−11.8
26	11 22.5	4909.94	−10.7	997	4906.94	3.00	−1.2	−11.9
27	11 23.1	4909.96	−11.0	997	4906.94	3.02	−1.1	−12.1
28	12 23.6	4910.98	−14.6	998	4910.91	0.07	+2.9	−11.7

The extraordinarily high accuracy of Campbell's observations appears from the good agreement of the reduced velocities *R*. The probable error of the result of one plate comes out for the observations in 1896, ±0.31 km, for those in 1899, ±0.26 km.

Mean: **R = —17.97 km, for 1896, Oct. 17.3** ($3850^{d}.3$),
R = —11.75 km, for 1899, Aug. 28.6 (4895.6).

III. OBSERVER: FROST.

(ASTROPHYSICAL JOURNAL, **10**, 184, 1899.)

No.	Gr. M. T.	Julian	V	Minimum No.	Minimum Epoch	A	B	R
	h	d	km		d	d	km	km
1	1899 Aug. 10 20.8	4877.87	−12.0	989	4875.20	2.67	−2.6	−14.6
2	Sept. 20 19.2	4918.80	−17.7	999	4914.88	3.92	+3.0	−14.7
3	27 16.0	4925.67	−10.6	1001	4922.82	2.85	−1.9	−12.5

Mean: **R = − 13.93 km, for 1899, Sept. 9.4** ($4907^{d}.4$).

The difference of 2.18 km between the results of Campbell and Frost of nearly the same date is noticeable, and leads us to infer a systematic error in one of the two series of observations —probably in that of Frost. The difference is, nevertheless, so small that we may recognize in the agreement of the two results a sure proof of the correctness of the value of *R*.

This series of observations exhibits the peculiarity that the application of the corrections does not improve the agreement of the separate results, but on the contrary makes it decidedly worse. The five successive observations Nos. 12 to 16, for instance, should show clearly the periodic variation of *V;* but instead of this *V* was found to be very nearly constant while the periodicity now comes out strongly in the values of *R*. I believe this can be explained as follows: The velocity of *Polaris* relative to the Earth at the time of these observations was very nearly zero, so that the stellar spectrum could show only a slight diplacement from the comparison spectrum. In the method of coincidences employed by Bélopolsky such small displacements are very difficult to measure, and the observer is hence caused to derive almost the same displacement, zero, on the probably

IV. OBSERVER: BÉLOPOLSKY.

(*Astron. Nach.* **152**, 199.)

No.	Gr. M. T.			Julian	V	Minimum No.	Minimum Epoch	A	B	R
				d	km		d	d	km	km
1	1899	Nov.	23.34	4982.34	− 8.5	1016	4982.34	0.00	+3.0	− 5.5
2			29.25	4988.25	− 6.9	1017	4986.31	1.94	−2.5	− 9.4
3			30.34	4989.34	− 7.1	1017	4986.31	3.03	−1.0	− 8.1
4		Dec.	3.30	4992.30	− 8.2	1018	4990.28	2.02	−2.6	−10.8
5			19.37	5008.37	− 8.2	1022	5006.15	2.22	−2.9	−11.1
6	1900	Jan.	3.24	5023.24	−10.6	1026	5022.02	1.22	−0.4	−11.0
7			12.24	5032.24	− 7.9	1028	5029.96	2.28	−3.0	−10.9
8			14.21	5034.21	−10.7	1029	5033.93	0.28	+2.6	− 8.1
9			15.28	5035.28	− 8.7	1029	5033.93	1.35	−0.8	− 9.6
10			16.22	5036.22	− 5.5	1029	5033.93	2.29	−3.0	− 8.5
11			18.22	5038.22	− 8.1	1030	5037.90	0.32	+2.5	− 5.6
12		March	22.43	5101.43	−10.7	1046	5101.39	0.04	+3.0	− 7.7
13			23.41	5102.41	− 9.9	1046	5101.39	1.02	+0.3	− 9.6
14			24.41	5103.41	−11.7	1046	5101.39	2.02	−2.6	−14.3
15			25.40	5104.40	− 9.6	1046	5101.39	3.01	−1.1	−10.7
16			26.37	5105.37	−10.4	1047	5105.36	0.01	+3.0	− 7.4
17			30.43	5109.43	− 9.7	1048	5109.33	0.10	+2.9	− 6.8

insufficiently sharp plates taken on successive days. The value of V arises for the most part from the reduction to the Sun (-12 km). Bélopolsky accordingly draws the correct conclusion that on account of their great uncertainty his results can hardly contribute to our knowledge of the changes in the velocity of the star. A probable error ± 1.5 km is found for the value of R and V given in the above table, which for the most part give the mean of two plates taken on the same day.

Mean: R = −9.12 km, for 1900, Jan. 26.2 (5046^{d}.2).

This mean value differs from Campbell's observations as well as from those of my own, next to be discussed, in the sense that the amount of the negative velocity was found too small by about 3 km by Bélopolsky. Since his observations of *ζ Geminorum*[1] deviate in the same direction from Campbell's, we may conclude that the spectrograms used by Bélopolsky are affected by very appreciable systematic errors.

[1] See CAMPBELL, "The Motion of *ζ Geminorum* in the Line of Sight," ASTROPHYSICAL JOURNAL, 13, 90, 1901.

V. OBSERVER: HARTMANN.

(See Table I.)

No.	Gr. M. T	Julian	V	Minimum No.	Minimum Epoch	A	B	R
		d	km		d	d	km	km
1	1900 March 7.42	5086.42	−15.1	1042	5085.52	0.90	+0.7	−14.4
2	9.44	5088.44	−14.2	1042	5085.52	2.92	−1.6	−15.8
3	10.42	5089.42	−13.1	1042	5085.52	3.90	+3.0	−10.1
4	11.31	5090.31	−11.7	1043	5089.49	0.82	+0.9	−10.8
5	14.48	5093.48	−17.1	1044	5093.45	0.03	+3.0	−14.1
6	21.49	5100.49	−12.7	1045	5097.42	3.07	−0.8	−13.5
7	April 2.38	5112.38	−13.8	1048	5109.33	3.05	−0.9	−14.7
8	3.37	5113.37	−17.7	1049	5113.30	0.07	+2.9	−14.8
9	4.36	5114.36	−15.3	1049	5113.30	1.06	+0.2	−15.1
10	5.42	5115.42	− 9.8	1049	5113.30	2.12	−2.8	−12.6
11	23.40	5133.40	−16.4	1054	5133.14	0.26	+2.7	−13.7
12	24.45	5134.45	−13.6	1054	5133.14	1.31	−0.6	−14.2
13	25.36	5135.36	− 4.7	1054	5133.14	2.22	−2.9	− 7.6
14	Nov. 7.33	5331.33	−14.2	1103	5327.58	3.75	+2.8	−11.4
15	8.25	5332.25	−12.8	1104	5331.55	0.70	+1.3	−11.5
16	8.38	5332.38	−14.0	1104	5331.55	0.83	+0.9	−13.1
17	8.69	5332.69	−12.6	1104	5331.55	1.14	−0.1	−12.7
18	9.22	5333.22	− 9.7	1104	5331.55	1.67	−1.8	−11.5
19	Dec. 3.40	5357.40	− 9.6	1110	5355.36	2.04	−2.7	−12.3
20	1901 Jan. 8.23	5393.23	−10.7	1119	5391.08	2.15	−2.8	−13.5
21	8.36	5393.36	−11.5	1119	5391.08	2.28	−3.0	−14.5
22	9.19	5394.19	−12.4	1119	5391.08	3.11	−0.5	−12.9
23	9.37	5394.37	−13.9	1119	5391.08	3.29	+0.8	−13.1
24	10.18	5395.18	−17.3	1120	5395.04	0.14	+2.9	−14.4
25	11.17	5396.17	−13.5	1120	5395.04	1.13	0.0	−13.5
26	11.39	5396.39	−12.9	1120	5395.04	1.35	−0.8	−13.7
27	14.18	5399.18	−15.4	1121	5399.01	0.17	+2.8	−12.6
28	14.37	5399.37	−15.9	1121	5399.01	0.36	+2.4	−13.5
29	15.18	5400.18	−12.6	1121	5399.01	1.17	−0.2	−12.8
30	16.18	5401.18	− 9.5	1121	5399.01	2.17	−2.9	−12.4
31	16.40	5401.40	−11.2	1121	5399.01	2.39	−3.0	−14.2
32	17.18	5402.18	−12.0	1121	5399.01	3.17	0.0	−12.0
33	17.44	5402.44	−15.3	1121	5399.01	3.43	+1.8	−13.5
34	18.35	5403.35	−15.1	1122	5402.98	0.37	+2.4	−12.7

These observations should be separated into two groups. The plates obtained in March and April 1900, when the spectrograph was exposed to all the changes of temperature, do not show a satisfactory agreement, as has been already mentioned above. At these dates the probable error of a single plate is ± 1.6 km. The largest deviation is shown by the plate of April 25, 1900. In the observation book it is recorded: "At a sudden

clearing up the observation was very hastily begun. In consequence of this the star was not well set at the middle of the slit; the star spectrum lies far toward the side on the comparison spectrum, and cannot be measured with certainty. There would be good reason for excluding this plate, but the others would nevertheless give a probable error of ± 1.2 km, which is decidedly large in view of the great accuracy of the measures on each individual plate. The exceedingly good effect of the attachment of the thermostat on the agreement of the observations can be seen by a glance at the values of R—the probable error of a plate since November 1900, is but ± 0.49 km. This shows clearly enough how imporant it is that the spectrograph be kept at constant temperature during an exposure.

It may be seen from these figures that the plates now obtained with spectrograph III possess a very high degree of accuracy, although they are still a little inferior to those secured by Campbell with the Mills spectrograph, which have not yet been equaled by plates elsewhere. The reason why my plates have not hitherto further increased in accuracy appears to lie in a small systematic error which usually clearly appears on evenings when more than one plate is made. The following summary gives the comparison of the values of R obtained on such evenings.

	First plate	Second plate	II — I
	km	km	km
1900 Nov. 8......	−11.5	−13.1	−1.6
1901 Jan. 8......	−13.5	−14.5	−1.0
9......	−12.9	−13.1	−0.2
11......	−13.5	−13.7	−0.2
14......	−12.6	−13.5	−0.9
16......	−12.4	−14.2	−1.8
17......	−12.0	−13.5	−1.5

The second plate on each of the seven evenings accordingly shows a stronger negative motion than the first, and there can be no doubt that there has been a disturbing source of error dependent either upon the time or the hour-angle of the star. I hope to succeed in discovering this error and in suppressing

it, which would obviously greatly increase the accuracy of the results. As long as this unknown source of error continues, all the measured velocities may be uncertain by a small systematic amount.

As my observations are separated by considerable intervals of time, I combine them in the three following mean values:

R = — 13.18 km	**for 1900 March**	**29.3**	($5108^d.3$)
R = — 12.07	**1900 Nov.**	**12.5**	(5336.5)
R = — 13.29	**1901 Jan.**	**13.1**	(5398.1)

The first of these is to be regarded as uncertain, as stated above.

In attempting to get a preliminary survey of the progress of the secular motion from the data collected here, we should note the following points. The Potsdam observations of 1888 may indeed, as experience has shown, be affected by systematic errors of several kilometers, but the great value that they possess on account of their early date is not diminished. But Frost's result, which is based on only three observations, does not come into consideration in comparison with the simultaneous observations of Campbell. I also omit the results of Bélopolsky's plates, on account of the possibility of large systematic errors, as well as those of my own plates made without the thermostat. There remain the following values of the secular motion:

1888	November 25,	$R =$ — 25.35 km	(Vogel and Scheiner)
1896	October 17,	— 17.97	(Campbell)
1899	August 29,	— 11.75	(Campbell)
1900	November 12,	— 12.07	(Hartmann)
1901	January 13,	— 13.29	(Hartmann)

It appears from these figures that the reversal of the secular motion has occurred since 1899: the negative velocity, which grew smaller since 1888, is now on the increase. The star must be followed spectroscopically for years for the accurate determination of this motion.

But *Polaris* may be also an interesting object for direct micrometer measures. If from the values found for the secular

motion we make the very rough estimate that the visible star in common with its invisible companion describes an orbit about a third body in about fifteen years with a velocity of some 6 km, then a simple computation shows that the diameter of this orbit must be at least three times that of the Earth's orbit. Hence it follows that in the course of that long period, the star must undergo changes of position which at least reach six times the value of its parallax. If we take Peters' value of $0\overset{''}{.}07$ for the parallax we get an amplitude of $0\overset{''}{.}4$ for the periodic variations in the position of the star, an amount which is sufficient to make itself felt even in absolute determinations of position.

Minor Contributions and Notes

THE PUBLICATIONS OF THE ALLEGHENY OBSERVATORY.

The Allegheny Observatory was founded in 1859 by the Allegheny Astronomical Society, a body composed of a number of private citizens of Allegheny and Pittsburg. For several years after its completion the Observatory, and the 13-inch telescope with which it was equipped, was used by the various members of the society and their friends, in observing the physical features and appearances of the Moon, planets, satellites, double stars, etc. But no regular or systematic scientific work appears to have been undertaken. In June 1867 the Observatory property was transferred under deed of trust to the Western University of Pennsylvania, and in August of the same year the board of trustees of the university elected Professor Samuel Pierpont Langley as the first regular director of the Observatory.[1]

The long and brilliant line of astronomical and astrophysical investigations and discoveries that have been made at the Allegheny Observatory under the directorship of Professor Langley and his no less able and distinguished associate and successor, the late lamented Professor James E. Keeler, have entitled this institution to a position among the leading observatories of the world. The Observatory, unfortunately both for itself and others, has not heretofore had the means to publish under its own auspices, the results of its work, and the records of its investigations and discoveries have, as a consequence, never been brought together in a collected form, but are scattered through many scientific journals and magazines, the proceedings and transactions of different scientific societies of this country and of Europe, and various official publications of the United States government, and the Smithsonian Institution. To obtain a complete and continuous record of the past work of our Observatory I have recently taken great pains to compile what I believe to be a full and accurate list of all the scientific papers

[1] For further details concerning the early history of the Allegheny Observatory see address by Mr. Brashear, "The Allegheny Observatory," *Pop. Ast.*, **8**, 541, Dec. 1900.

that have been published from the Observatory from 1869 to the end of 1900, by the various members of its staff. The history of the scientific activity of the Observatory during these thirty-one years of work is contemporaneous and closely identified with the history of the development of the "new astronomy," and this list of papers may therefore be of interest and value to the student of astrophysics. For this reason, and for the benefit of our exchanges, I have decided to publish it, together with a brief statement concerning the plans that have been formed in regard to future publications.

The papers in the following list are arranged as nearly as possible in the order of the date of publication. To save unnecessary repetitions of names, titles, etc., the following abbreviations have been used:[1]

S. P. L. Professor S. P. Langley. (Director of the Observatory, 1867–1891.)

J. E. K. Professor James E. Keeler. (Assistant Astronomer, 1881–1883; 1884–1886; Director of the Observatory, 1891–1898.)

F. W. V. F. W. Very. (Assistant to the Director, 1878–1890; Adjunct Professor of Astronomy, 1890–1895.)

J. A. B. John A. Brashear. (Acting Director of the Observatory, 1898–1899; Chairman Observatory Committee.)

F. L. O. W. Professor F. L. O. Wadsworth. (Director of the Observatory, 1900 —.)

The abbreviations for the titles of journals, proceedings of societies, etc., are the same as have been adopted by the ASTROPHYSICAL JOURNAL (see Vol. II, pp. 396–400, Dec. 1895), viz:

A. and A.	*Astronomy and Astrophysics.*
Am. Jour. Sci.	*American Journal of Science and Arts.*
Am. Met. Jour.	*American Meteorological Journal.*
Wied. Ann.	*Annalen der Physik und Chemie.*
Ann. Chim. et Phys.	*Annales de Chimie et de Physique.*
Ast. Jour.	*Astronomical Journal.*
A. N.	*Astronomische Nachrichten.*
Ap. J.	*Astrophysical Journal.*
Bull. Astr.	*Bulletin Astronomique.*
Bull. Phil. Soc.	*Bulletin of the Philosophical Society of Washington.*

[1] The list comprises only papers of a scientific or technical character. A number of popular articles, written by Professor Langley for the Pittsburg, New York, and London daily papers, are not included, and several reviews of the work of other investigations, written by Professor Keeler, have likewise been omitted for the reason that they had no reference or relation to the work of the Observatory.

C. R.	*Comptes Rendus de l'Academie des Sciences.*
Jour. B. A. A.	*Journal of the British Astronomical Association.*
Jour. Franklin Inst.	*Journal of the Franklin Institute.*
Mem. Nat. Acad.	*Memoirs of the National Academy of Sciences* (Washington).
Mem. R. A. S.	*Memoirs of the Royal Astronomical Society.*
Mem. Spettr. Ital.	*Memorie della Società degli Spettroscopisti Italiani.*
M. N.	*Monthly Notices of the Royal Astronomical Society.*
Nat.	*Nature.*
Obs'y.	*Observatory.*
Phil. Mag.	*Philosophical Magazine.*
Photo. Times.	*Photographic Times.*
Pop. Astron.	*Popular Astronomy.*
Proc. American Acad.	*Proceedings of the American Academy of Arts and Sciences.*
Proc. R. Inst.	*Proceedings of the Royal Institution of Great Britain.*
Proc. R. Soc.	*Proceedings of the Royal Society of London.*
Pub. A. S. P.	*Publications of the Astronomical Society of the Pacific.*
Report A. A. A. S.	*Report of the American Association for the Advancement of Science.*
Report B. A. A. S.	*Report of the British Association for the Advancement of Science.*
Sid. Mess.	*Sidereal Messenger.*
Trans. Ast. Phys. Soc. Toronto.	*Transactions of the Astronomical and Physical Society of Toronto.*

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159. The Importance of Astrophysical Research and the Relation of Astrophysics to Other Physical Sciences, Address Delivered at the Dedication of the Yerkes Observatory, by J. E. K.; *Ap. J.*, **6**, 271–288, Nov. 1897; *Science*, Nov. 1897.
160. Spectra of stars of Secchi's Third Type, by J. E. K.; *Ap. J.*, **6**, 423–424, Dec. 1897.
161. Some Notes on the Application of Photography to the Study of Celestial Spectra, by J. E. K.; *Photo. Times*, **20**, 197–203, May 1898.
162. Allegheny Observatory, Annual Report of the Director for the Year ending Mar. 31, 1898, by J. E. K.; 4 pp., 8vo, May 1898.

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163. Allegheny Observatory, *Report of the Acting Director* for the Year ending Mar. 31, 1899, by J. A. B.; 2 pp., 8vo, June 1899.
164. Results of an Examination of Spectrograms of α *Orionis* obtained during the Recent Irregular Minimum, by Henry Harrer; *Ap. J.*, **10**, 290–291, Nov. 1899.
165. Astronomical and Engineering Equipment of the New Allegheny Observatory, by F. L. O. W.; Address delivered before the Engineer's Soc. of Western Pennsylvania.; *Proc. of the Soc.*, **16**, No. 3, Mar. 1900.
166. Allegheny Observatory, *Report of the Acting Director* for the Partial Year ending Dec. 31, 1899, by J. A.B.; 2 pp., 8vo, June 1900.
167. Allegheny Observatory, Annual *Report of the Director* for the Year ending Mar. 31, 1900, by F. L. O. W.; 10 pp., 8vo, June 1900.
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169. Plans and Elevations of the New Allegheny Observatory, by F. L. O. W., and T. E. Billquist; *The Brickbuilder*, **9**, 3 plates, quarto, June 1900.
170. Atmospheric Radiation, by F. W. V.; U. S. Weather Bureau, *Bulletin G.*, 134 pp., quarto, 1900.
171. Photographs of the Solar Eclipse of May 28, 1900, by the Allegheny Observatory Eclipse Expedition; *Pittsburg Bulletin*, **41**, 1–2, Oct. 1900.
172. James Edward Keeler, A Biographical Tribute, by J. A. B.; *Pop. Astron.*, **8**, 476–481, Nov. 1900.
173. The Allegheny Observatory, Address delivered at the laying of the Corner Stone of the new Observatory, Oct. 20, 1900, by J. A. B.; *Pop. Astron.*, **8**, 541–550, Dec. 1900.

It is no longer possible to furnish or to obtain complete sets of these papers. Indeed, as far is known here, there are only one or two complete collections in existence. We still have copies of those papers marked with a star [*] in the preceding list, *i. e.*, of Nos. 1, 6, 9, 10, 11, 14, 15, 16, 17, 18, 19, 20, 26, 30, 35, 36, 37, 41, 42, 45, 46, 49, 51, 53, 54, 56, 58, 59, 60, 61, 62, 66, 67, 68, 69, 70, 71, 72, 75, 76, 77, 78, 80, 82, 87, 90, 91, 92, 98, 101, 102, 103, 112, 115, 116, 117, 119, 129, 133, 137, 138, and 141.

We shall be very glad to send copies of any of the above to our past contributors to enable them to complete their collections as far as is now possible. Applications for the separate numbers will, in general, be filled in the order in which they are received, until the supply of each is exhausted, but as the number of copies is in many cases

limited, we must, when it is necessary, give the preference to those to whom the Observatory is most indebted for past contributions.

PUBLICATIONS OF THE NEW OBSERVATORY.

The future publications of the Allegheny Observatory will be divided into two series: (1) *The Annals of the Allegheny Observatory*, in the form of quarto volumes of uniform size and binding, containing detailed descriptions of instruments, methods of work and results of special researches and continued series of observations; and (2) *Miscellaneous Scientific Papers of the Allegheny Observatory*, consisting of reports, special announcements, bulletins, and reprints of papers communicated to the various scientific journals and societies.[1] The successive papers of the second series will be numbered consecutively in the order of the date of publication, and will be issued as far as possible in uniform octavo size, 6 inches by 9 inches, so that they may subsequently be bound together if desired in volume form. It will probably be impossible or inexpedient to reprint all of the shorter and less important communications to journals and societies in sufficient numbers for complete exchange and distribution, but in such cases, for the sake of uniformity and continuity of record, numbered title pages only giving the place and date of publication of the article will be issued.

In conclusion, I take this opportunity to extend on the part of the Observatory, our most sincere thanks to all of those who have so kindly and generously contributed to our library in the past, and to cordially invite all of those who have not already done so to arrange for an exchange of publications in the future.

A detailed acknowledgment of the more recent and valuable gifts to the library, and as far as possible a complete list of our contributors and exchanges, will be published in the first volume of our *Annals*, which it is intended to devote to a full description of our new Observatory buildings, instruments, library, and general equipment; together with a short history of the administration and scientific work of the old Observatory. Part of the manuscript of this volume is already prepared, and it will be issued as soon as the new Observatory and its principal instruments are completed.

F. L. O. WADSWORTH.

ALLEGHENY OBSERVATORY,
May 1901.

[1] The present paper forms No. 3 of this series.

SPECTRUM OF *NOVA PERSEI* NO. 2.[1]

THE star *Nova Persei* No. 2, has now become so faint that its spectrum can no longer be photographed satisfactorily with a large dispersion. Moreover, it is, at sunset, so low in the northwest that it cannot be observed to advantage in the evening. A series of photographs of its spectrum, ever since its appearance, has been made with the 11-inch Draper telescope. At first, two prisms were used, giving a spectrum in which the distance from $H\epsilon$ to $H\beta$ is 3.76 cm. After March 19 it was found that, owing to the diminishing light, better results could be obtained with one prism, giving a spectrum in which the distance from $H\epsilon$ to $H\beta$ is 1.80 cm. Meanwhile, a second series of spectra has been obtained with the 8-inch Draper telescope with much smaller dispersions, the distances between the lines $H\epsilon$ and $H\beta$ being 0.57 cm., and 0.14 cm., respectively. Care was taken, when the *Nova* was bright, to allow the spectrum to trail, or to give it a rapid motion over the plate, so that it should not be over-exposed with these small dispersions. These photographs will, therefore, be comparable with those taken when the star becomes very faint. It is expected that the spectrum can thus be studied even when the star is of the tenth magnitude or fainter, although its light will have diminished more than ten thousand times. The series of spectra taken with the 11-inch Draper telescope is now completed, unless the star should again become bright. A careful study of these photographs has been made by Miss Annie J. Cannon and will be published in the *Annals*. A brief summary of the results is given below. The principal bright lines are accompanied by dark lines on the edge of shorter wave-length. All of the wave-lengths have been referred to the centers of these bright lines, since after March 23 the dark hydrogen lines disappeared.

A description of the spectrum of *Nova Persei* No. 2, as photographed here on February 22, 23, and 24, 1901, is given in *Circular* No. 56. Plates taken since February 24 show numerous changes. Narrow dark lines, probably due to reversal, appeared upon the bright bands, and the latter increased in intensity with respect to the continuous spectrum. The dark bands became narrower, and, in some cases, separated into two or more parts. A peculiar dark band between $H\eta$ and $H\zeta$, and extending from λ 3845 to λ 3856, which was as intense as $H\zeta$ on Februrary 24, faded very rapidly and was not seen after February

[1] *Harvard College Observatory Circular* No. 59.

28. A dark band near $H\delta$, and having wave-length 4056 to 4069 on February 24, showed peculiar changes in intensity, width, and wave-length. The dark bands of hydrogen became double, and then grew fainter, and in place of the hazy bands two narrow, dark lines appeared. The dark band K also became double and decreased in intensity. On March 17, $H\zeta$, $H\epsilon$, $H\delta$, and $H\gamma$, each consisted of a narrow, sharply defined, dark line, and on the most careful inspection, an extremely faint component, well separated, was seen towards the violet. $H\beta$ was clearly double, the faint component being well marked. The wide dark band, K, had entirely disappeared, and a narrow dark line remained, which was only slightly more intense than the reversed K.

On March 19 there appears to have been a peculiar change in the spectrum. No dark lines were present, except the fine lines due to reversal superposed on the bright bands, and the continuous spectrum was almost invisible. The line K was absent. The plate on this date was taken with two prisms and exposed 120^{m}. A peculiar broadening, or displacement towards the violet, of the bright band $H\zeta$ had taken place since March 17. On March 19 this band extended more than half way to $H\eta$. On March 23 the continuous spectrum was present and narrow dark lines were seen on the edges of shorter wave-length of the bright light lines $H\epsilon$, $H\delta$, and $H\gamma$. Three other dark bands were present, and several bright bands besides those of hydrogen. The position of $H\zeta$ was normal. On March 27 a strong continuous spectrum was seen, but the dark components of the hydrogen bands were absent, and have not been seen since. A well marked, narrow, dark line was present at wave-length 3865, and a fainter dark line at λ 3860. There was a hazy, dark band extending from λ 3806 to λ 3827, and a bright band from λ 4453 to λ 4489, both of which appeared to coincide with helium lines. The position of $H\zeta$ was normal. On March 30 the spectrum was like that of March 27, except that line λ 3865 was more intense, and λ 3860 was not seen. The continuous spectrum, which was very intense, was photographed far into the violet, but no lines were distinctly seen beyond a hazy, dark band which extended from wave-length 3775 to 3794. The bright band λ 4908 to λ 4942 was very faint, and the band λ 4990 to λ 5040 was brighter than the magnesium band, *b*. On April 1 the spectrum was nearly the same as on March 30. Owing to a long period of cloudy weather, no good plates were obtained from April 1 to April 12, when a peculiar spectrum was photographed. $H\zeta$ appeared to be missing, and near its place there

was a bright band as intense as $H\beta$, whose center was λ 3875. This band, $H\epsilon$ and $H\delta$, were very sharply defined towards the red, and hazy towards the violet. It is of interest to note that a dark band of well-marked intensity occurs in the spectrum of γ *Velorum* at wave-length 3875. A peculiar bright line appeared in the *Nova* on April 12 near $H\gamma$, on the side of greater wave-length. The wave-length of this line is 4384, and it was quite sharply defined towards the red, while the space between it and the bright band $H\gamma$ appeared somewhat like a faint bright band. On April 13 the position and intensity of $H\zeta$ were normal, and, although the plate was poor, the spectrum was not of the peculiar type of April 12. This peculiar spectrum was, however, photographed again on April 26, when the peculiarities were even more marked than on April 12. Band λ 3875 was the most intense bright band on April 26. Band λ 4990 to λ 5040 had also increased in intensity, while $H\delta$ and $H\beta$ had diminished. $H\gamma$ was more intense than $H\beta$. The continuous spectrum was absent. Bands λ 4908 to λ 4942, and the magnesium band, *b*, were not seen. On April 27 the spectrum was again normal. Band λ 3875 was absent, and the continuous spectrum was of well-marked intensity. In general, the spectrum appeared to be like that of March 30. On April 28 the spectrum was again peculiar, and like that of April 26. The bright bands, which were very sharply defined on the edge of greater wave-length on April 12, were on April 26, sharply defined on the edge of shorter wave-length. When the plate taken on April 12 is superposed upon that of April 26, this difference in the definition of the bands is very striking.

On May 1 and 3 the spectrum was peculiar and like that of April 28. Photographs were attempted on May 6 and 7, but the *Nova* was evidently too low and the glare of sunset prevented success. Nothing is seen on the plates. It thus appears that two types of spectra have been visible from March 19 to May 3. Photographs of the peculiar spectrum were obtained on March 19, April 12, 26, 28, May 1 and 3; of the normal spectrum on March 23, 27, 30, April 1, 13, and 27. It is interesting to note the connection between the changes in the spectrum and in the light of the *Nova*, for on all the dates on which the peculiar spectrum was photographed, a minimum occurred, according to the Harvard visual and photometric observations, except on April 12. The magnitude of the star was the same on April 12 and 13, while the spectrum was different. On April 26 and 28, however, there occurred marked minima, and the peculiar spectrum was photographed on

both dates, while on the intermediate evening, April 27, when the *Nova* was about a magnitude and a half brighter than on either the preceding or following night, the spectrum was normal.

The following table shows the connection between the spectrum and the magnitude of the *Nova*. The first column gives all the dates on which the *Nova* was successfully photographed at Harvard with the 11-inch telescope from March 17 to May 3, 1901. The second column gives the character of the spectrum. The third column gives the magnitude from visual observations reduced to the photometric scale.

RELATION OF SPECTRUM TO MAGNITUDE.

Date	Spectrum	Mag.	Date	Spectrum	Mag.	Date	Spectrum	Mag.
March 17	Normal	3.8	April 1	Normal	4.1	April 27	Normal	4.2
March 19	Peculiar	5.0	April 12	Peculiar	4.6	April 28	Peculiar	5.4
March 23	Normal	3.6	April 13	Normal	4.6	May 1	Peculiar	5.3
March 27	Normal	4.1	April 26	Peculiar	5.8	May 3	Peculiar	5.5
March 30	Normal	4.2						

THE SPECTRUM OF η *Carinae.*

On April 16, 1898, Miss Cannon, while examining plates taken in Arequipa for the classification of stellar spectra, recorded the statement regarding *Nova Aurigae*, "Spectrum made up almost entirely of bright bands that coincide in position with those of η *Carinae*." This is followed by a detailed description of the spectrum of η *Carinae*, and a comparison with the spectrum of *Nova Aurigae*, including the bright and dark hydrogen lines, which is incorporated in the "Remarks" in the *Annals* Vol. XXVIII, p. 175, now in the hands of the printer. Photographs pointing out this resemblance were contained in the exhibit of the Harvard Observatory at Paris in 1900, and are now in the Buffalo Exhibition.

EDWARD C. PICKERING.

June 6, 1901.

NOVA PERSEI, NO. 2.

AN examination of the Draper photographs of the spectra of *Nova Persei*, No. 2, by Mrs. Fleming, shows that, like other *novae*, it has been gradually changing into a gaseous nebula. The resemblance to the nebula *N. G. C. 3918* is now so close that in a photograph taken

on June 19, 1901, no marked difference was noted, except that the nebular line λ 5007 is about eight times as bright as $H\beta$ in the nebula, and only equal to it in the *Nova*. The lines λ 3869, 3970 ($H\epsilon$), 4102 ($H\delta$), 4341 ($H\gamma$), 4688, 4862 ($H\beta$), 4959, and 5007, are common to both and, except the last, have nearly the same relative intensities. Four bright lines between $H\gamma$ and $H\beta$ appear faintly in the *Nova* and are not present in the nebula, while one, λ 4364, is seen in the nebula but not in the *Nova*, perhaps owing to the proximity of $H\gamma$.

EDWARD C. PICKERING.

June 25, 1901.

NOTICE.

The scope of the ASTROPHYSICAL JOURNAL includes all investigations of radiant energy, whether conducted in the observatory or in the laboratory. The subjects to which special attention will be given are photographic and visual observations of the heavenly bodies (other than those pertaining to "astronomy of position"); spectroscopic, photometric, bolometric, and radiometric work of all kinds; descriptions of instruments and apparatus used in such investigations; and theoretical papers bearing on any of these subjects.

In the department of *Minor Contributions and Notes* subjects may be discussed which belong to other closely related fields of investigation.

Articles written in any language will be accepted for publication, but unless a wish to the contrary is expressed by the author, they will be translated into English. Tables of wave-lengths will be printed with the short wave-lengths at the top, and maps of spectra with the red end on the right, unless the author requests that the reverse procedure be followed. If a request is sent *with the manuscript* one hundred reprint copies of each paper, bound in covers, will be furnished free of charge to the author. Additional copies may be obtained at cost price. No reprints can be sent unless a request for them is received before the JOURNAL goes to press.

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